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IN THE HIGH HEAVENS

MARS IN 1877.
From Mr. Green's Drawings. Memoirs of the Royal Astronomical Society.

IN THE HIGH HEAVENS

BY

SIR ROBERT S. BALL, D.Sc., LL.D., F.R.S.

LOWNDEAN PROFESSOR OF ASTRONOMY AND GEOMETRY IN THE UNIVERSITY OF CAMBRIDGE

AUTHOR OF "IN STARRY REALMS," "GREAT ASTRONOMERS," ETC.

WITH NUMEROUS ILLUSTRATIONS

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PREFACE TO THE NEW EDITION.

I HAVE revised this volume for the present edition. It was impossible not to notice in the chapter on the Fifth Satellite of Jupiter that still further discoveries in the same direction had been made since that chapter was originally written. The latter part of the chapter on the Boundaries of Astronomy is mainly an endeavour to weigh the arguments for and against the truth of the nebular theory of the origin of the Solar System. I may, therefore, here say that further consideration of the subject only tends to make me feel more confident that the nebular theory does really express the law of nature.

ROBERT S. BALL.

CAMBRIDGE,

21st *February*, 1906.

PREFACE TO THE FIRST EDITION.

THE present volume contains a series of sketches of certain parts of astronomy which are now attracting a great deal of attention. I may specially mention among the novel parts of the book Chapters XIII. and XIV., relating to Meteorites. The last chapter, "On the Constitution of Gases," discusses points which seem destined to be of great importance in astronomy, while the first chapter studies, in a new light, the great question of the movement of our system.

Although several of the chapters consist of articles contributed to the *Contemporary* or the *Fortnightly*, yet it has been necessary to revise them in accordance with the advance of science. In certain cases considerable alterations have been found necessary. The opportunity has also been taken to provide for the illustration of the articles, which was not possible under the circumstances of their original appearance.

It gives me much pleasure to acknowledge the kindly assistance which has been afforded. I am indebted to Dr. Downing, the Superintendent of the "Nautical Almanac," for permission to reproduce the path of the Eclipse of 1893, to Mr. N. Green for his beautiful drawing of Mars, and to Mr. J. R. Gregory for the photograph of the large meteorite found at Youndegin, West Australia. I must finally express my thanks to my friends, the Rev. Maxwell H. Close and Mr. L. E. Steele, who have read the proofs through.

ROBERT S. BALL.

OBSERVATORY, CAMBRIDGE,
21st *September*, 1893.

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CHAPTER I.

THE MOVEMENTS OF THE SOLAR SYSTEM.

I HAVE often speculated as to the appearance which the heavens must have presented at very remote epochs. I do not now merely refer to such epochs as those to which human history extends. There can be no great difference between the aspect of the skies now and the aspect which they presented when Ptolemy or Hipparchus observed them. No doubt we must admit that some changes have taken place, for change is the law of nature. In a thousand years, or in a hundred years, or ten years, or even in one year, a number of alterations take place in the positions of the fixed stars which are quite perceptible to the refined measurements of the modern observatory, though they would not suffice to produce a derangement of the heavens large enough to be discernible by unassisted observation. But in the present chapter I specially want to consider the variations in the aspect of the heavens, which would be presented not merely after the lapse of a few centuries or a few tens of centuries, but after

stretches of time much longer still. We know from the testimony of the rocks that our earth has been the abode of living creatures for periods which it seems impossible to express in less than thousands of thousands of years. It therefore seems an interesting question to investigate the possible amount of transformation which the heavens have undergone in, let us say, a million of years.

The line of reasoning we shall employ suffices at all events to show how mighty is the transformation which has occurred within geological periods. The changes in the heavens are as profound as the changes in the earth. Let us consider the case of a star, or other celestial body, which moves through space at the constant rate of 20 miles a second. I have adopted this particular velocity as fairly typical of sidereal motions generally. It is rather larger than the speed with which the earth moves in its orbit. The velocities of many of the stars are, however, known to be quite as great as that which we have assumed. Indeed, in the case of many stars the speed is greatly in excess of 20 miles a second. There are several stars certainly known to be moving twice as fast; nor are speeds considerably higher than even this unobserved. We may, for instance, mention the famous star known to astronomers as Groombridge No. 1830, which hurries along at the rate of at least 200 miles a second. Indeed, in some cases stellar velocities are attained which appear to be even greater than that just mentioned. We do not therefore make any extravagant supposition in adopting a speed of 20 miles per second, as the basis of our calculation. This being granted it is now a simple problem to discover with sufficient approximation the change in apparent visibility which such a

body would undergo after the lapse of a stated period of time. Twenty miles a second means 1,200 miles a minute. That is of course 72,000 miles per hour, 1,728,000 miles per day, or 630,720,000 miles per annum. It therefore follows that in a million years the distance through which a star will move, on the assumption we have made, cannot be less than 600,000,000,000,000 miles.

I do not think that the effect of these considerations on the continuance of the visibility of stars throughout vast periods of time has always been quite fully appreciated. The figures given will provide a demonstration that there must have been a vast change in the appearance of the heavens within the lapse of the last million years. To carry the inquiry much farther, we ought, however, to be acquainted with the distance of each star under consideration, and this is an element of which we are ignorant in the great majority of cases. A single instance will, however, suffice for an illustration. The nearest star as far as we yet know in the northern hemisphere is 61 Cygni. There have been, it is true, some discrepancies between the various determinations of its distance which different astronomers have obtained. I think, however, that we cannot be far wrong in adopting a value of 50,000,000,000,000 miles. I shall therefore take this magnitude as a typical distance to which we may apply the arguments of the present chapter. It appears that in the course of a million years a star with the average speed of 20 miles a second would move over a distance which was about a dozen times as great as the distance between 61 Cygni and the solar system. It will be noted that in expressing the speed of

the star it was assumed that the solar system remained at rest. If, indeed, the solar system had a motion equal and parallel to that of the star, it would have been impossible to determine that of the latter, unless the motion of the solar system were itself known. It must, therefore, be borne in mind that the velocities with which we are at present concerned must be regarded as relative measurements conducted on the supposition that the solar system itself remained fixed.

If a star were displaced as much as a dozen times its original distance from the sun, it is obvious that a tremendous change in the apparent lustre of the star would be the consequence. If the body had moved directly away from the sun, the distance between the two objects would be increased in the proportion of thirteen to one. Had the star travelled in about the opposite direction, it would have passed comparatively near to the earth and its distance from the solar system, in the course of a million years, would be increased in the proportion of about eleven to one. On the other hand, if the star had moved in a direction perpendicular to the line from the star to the solar system, then the distance between the sun and the star would be rather more than twelve times as great as it was originally.

It is thus evident that whatever be the direction in which the star may happen to have moved, the distance between the solar system and the star will have increased during the lapse of a million years to nearly, if not quite, a dozen times as much as it was at the commencement of the same period. This leads to a remarkable conclusion with respect to the permanence of the visible heavens.

According to the laws of optics, the apparent lustre of

a star varies inversely as the square of its distance. If the distance of a star be doubled, then its brightness is decreased to one-fourth. If the star be removed to a distance three times its original amount, then the apparent brightness declines to one-ninth. We may therefore infer that if a star be withdrawn to a distance which is twelve times that which it has at present, then the lustre of the star will be reduced to one hundred and forty-fourth part of its primitive value. This reasoning shows that any star which is now situated at the distance and maintains the proper motion we have supposed, will in the course of a million years have its apparent lustre reduced to the one hundred and forty-fourth part of that which it now displays. It need hardly be said that such a reduction of brightness would be certainly sufficient to render most of the stars wholly invisible. Indeed, it is only stars which possess exceptional lustre at their present distance which would continue to be visible, even as telescopic objects, when they had suffered so serious a loss.

These considerations lead to consequences of a remarkable character. We have adopted an average value of the proper motions, and it appears that on such an assumption it is highly improbable that any two stars not physically connected as a binary pair, should remain in comparative proximity for a period so long as a million of years. The case may perhaps be conveniently likened to that of a number of ships which are each bent on their different courses. At any particular time each of these vessels will generally have several other ships in view, but as each moves on its way the distances gradually alter, so that in the course of an hour or two the vessels

have become so far dispersed that not one of those which was first seen is now above the horizon, while other ships, not at first to be discerned, have come within sight.

These considerations illustrate the transient nature of the appearance of the starry heavens when we contemplate periods of time comparable with those which the facts of geology demand for the requirements of earth-history. It is quite true that for the convenience of the argument I have been obliged to take specific numbers and to assume certain conditions, but there can be no doubt that the illustration is sufficient for demonstrating that in the course of the next million years the disposition of the sidereal heavens must present a totally different appearance from what it now shows. By the same reasoning we feel assured that if a view of the heavens had been obtained from this earth a million years ago, it must have been totally different from that offered by our present skies. Conceive that a man were transported back to the time when those great forests were flourishing whose remains have been preserved in the form of coal. It is, I believe, practically certain that few, if any, of the stars that now adorn our skies would be then discernible by him. I do not mean to say that there were no stars visible at the time of the coal forests. It would be much more reasonable to suppose that the firmament was as richly spangled with gems then as it is now, but those heavens would not be the heavens which we know. The estimates of geological chronology generally received would place the date of the great coal forests at an epoch far more remote than a million years back. Indeed, if anyone were to maintain that the remoteness of the period had to be expressed in

tens of millions of years, I do not know of any facts by which he could be contradicted. The longer the time the more complete would have been the transformation in the visible objects on the sky.

Except that the stars of this remote antiquity must have been totally different from our present stars, we know but little of them. Indeed, I might almost say we can know nothing. Possibly some of our telescopic nebulæ and clusters, whose distances are believed in many cases to be greatly in excess of the average distance of the stars, might not have been so totally transformed by their proper motion even in millions of years as to be unrecognisable by an eye familiar with the appearance they bear at present. There can, however, be no doubt that greatly as the stars may have changed the aspect they present to the terrestrial inhabitants, the sun, to which in the same time those inhabitants owe so much can have undergone but little appreciable alteration. The luxuriant vegetation of the coal measures demonstrates that the great luminary must have dispensed light as well as heat in those ancient days. The same fact is strikingly exemplified by the presence of eyes in extinct animals. Indeed, in some cases the eyes of creatures now only known to us by their fossil remains, seem to have been of the most elaborate character. Who that has ever visited any of our geological museums has not been interested in examining the great eye of the ichthyosaurus? In that unique organ of vision there is a remarkable apparatus of bony plates, apparently intended for adaptation of the organ to varying conditions. It is obvious that for some reason or other, as to which we can only speculate, a visual organ of excessive power and adaptability was required for this wondrous fish-reptile. I

have often, indeed, longed to know what must have been the aspect which the heavens presented to that strange creature provided with such a marvellous optical instrument. No doubt his eye was generally employed for a much more practical purpose than that of astronomical contemplation. If, however, an ichthyosaurus ever did spare a glance at the heavens, what would have been the sight that would have met his gaze?

The sun would have shone on his earth as on ours. The luminary was certainly larger then to some trifling extent than it is at present. It was, however, in all probability nearly as bright as it is now, though it is just possible that photometric measurements would have shown it to be not quite so lustrous as the orb we know. For, paradoxical as it may seem, there are grounds for believing that the sun, though on the whole losing heat, may nevertheless be waxing somewhat brighter and hotter, and radiating more fiercely than then. But this is a matter which at present we need not further pursue; suffice it now to say that there is not the least reason to think there could have been any *very considerable* change in the physical characteristics of the sun, as the ichthyosaurus saw it and as we see it now.

The moon, too, at that remote epoch must have run through just the same phases as it does at present. No doubt our satellite was somewhat nearer to the earth in those days than it is now. Its orb would, therefore, have seemed larger, and its periodic time would have been somewhat less. New moon must then have succeeded new moon at a somewhat briefer interval than at present. It is quite possible that the lunar craters which are now so completely extinct may not have exhausted

their pristine energy before the days in which the ichthyosaurus flourished. This circumstance would perhaps have made the telescopic picture of the moon of that period vastly more interesting than any views of our satellite which are now to be obtained. No very great difference would have been noticeable between the planets of this remote antiquity and the planets of our skies. Venus would then, as now, have gone through that beautiful series of changes from the evening star to the morning star, and the intervals would have been much the same as they are at present. The moons of Jupiter, his belts and his orbit, would offer no striking variation from the Jupiter as now disclosed to astronomers. The rings of Saturn would probably have been much the same then as now, though it may be admitted that certain changes in the details of the Saturnian system have been thought to be in progress. As to what may have been the condition of the planet Mars, some million of years ago, we really have no idea. It is not improbable that the face of that globe would then have been very different from the globe which we now see. But the orbit in which Mars revolved even a million years ago would not have differed widely from that which it now traverses.

Speaking generally, we may say that the appearance of the planets, at all events to the unassisted eye, and also the movements of these bodies, would have been not unlike the corresponding phenomena which they now exhibit. Doubtless then, as now, comets must from time to time have flashed across the heavens; doubtless also meteors and showers of shooting stars must have rained down, perhaps in even greater abundance than they do at present. Probably solid meteorites may

have landed on the earth even with greater frequency than in these latter days. But it must be admitted that in other respects the appearance of the heavens would have been totally different in the days of the ichthyosaurus from that which we now know. The Great Bear would not then have been discernible as the most striking group in the northern sky. Orion and the other well-known groups would not have yet come into vision. The pole would not then have been indicated by the pole star, and whatever may have been the brightest star in the heavens it is almost certain that it cannot have been Sirius. A zodiac there was, no doubt, but the signs by which it was to be marked were not the Ram and the Bull and the Heavenly Twins, or the other groups which have discharged that duty throughout the ages of human history. It may have been that the Milky Way was a luminous girdle around the heavens in the time of the ichthyosaurus, as it is at present. But it is certain that the general features of the heavens must have been profoundly modified between the long distant past and the present.

It may, therefore, be noted as a curious circumstance that the only permanent feature of our heavens in regard to such periods as those we are considering are not the fixed stars but the wandering planets. As they wandered then so they wander still, ever remaining members of that system over which the sun presides. It is no doubt impossible for us to form any conception as to what those stars or groups of stars may have been which adorned the skies of ancient geological times in the same way as our own constellations brighten our present skies. Calculations so instructive elsewhere will not suffice here. No methods known to us,

or conceivable by us, can ever reproduce what the heavens must have been like at periods of millions of years ago. There could be no more interesting sight than a glimpse at the starry heavens in the time of the ichthyosaurus. We have read somewhere of a fable to the effect that the last object on which an eye rested ere it closed imprinted its picture permanently on the retina. Would that such a notion were founded on fact, and that the impression of the last celestial picture on which the eye of the ichthyosaurus gazed before he breathed his last were treasured up in the fossilized organism.

In the consideration of the gradual transformation of the heavens, I have found it convenient to speak as if the earth, or rather the solar system to which the earth belongs, occupied a fixed position in space. But when we have learned that some or all of the stars are in movement, it seems right to inquire whether the sun might not also participate in the motion. May not the sun be engaged in some mighty voyage through the celestial spaces, taking in its company the earth and the other planetary bodies by which it is attended? Here is, indeed, a grand problem; I propose to enter into its discussion with the assistance of certain recent investigations.

In the first place, it must be remembered that for the sun to be actually devoid of movement could be little short of miraculous. There are, of course, an infinite number of different movements possible, for there is every degree of velocity and difference of direction to be considered. On the other hand there is only one type of rest, or absolute quiescence, and it would be just as likely that a body should possess any stated velocity, say ten miles, ten

yards, and three quarters of an inch per second, as that it should possess that particular characteristic implied in the absence of all movement whatsoever. It thus appears that, even in the absence of direct testimony on the subject, there is only one chance that the sun should be at rest, while the chances that it is not at rest are absolutely infinite. Under these circumstances rational beings will conclude that the sun is not at rest, and once we have admitted that the system is in motion, our next duty will be to discover the characteristics of that movement.

There are several different methods by which this problem has been investigated. They all lead to results which are in such substantial accordance that there can be no reasonable doubt that the difficult problem of the motion of the sun has been solved with considerable approximation.

It will be noted that in this inquiry there are two different problems which have to be considered. The first of these relates to the direction of the sun's movement, and the second to its velocity. These investigations have hitherto generally been conducted simultaneously. It is, however, now apparent that the most satisfactory solution of the problem is to be obtained by employing one of the methods for the determination of the direction of the sun's motion, and a quite different method for the determination of its velocity. I shall deal with the two branches of the subject consecutively.

The only method of learning the actual displacement of the solar system in space must be founded on the observation of bodies external to that system; at all events to the extent of not participating in the motion with which the system is animated. Of course the only such

bodies available are the stars. Here at once we are met by the difficulty that the stars are themselves in movement. These movements affect the value of the stars, as points of reference, so seriously, that if there were only one or two stars available for the inquiry it would be utterly impossible for us ever to discover the movements of the solar system. But there are, of course, hundreds, or rather thousands, of stars, which can be made to render assistance. No doubt these stars are themselves endowed with movements, but their journeys are so varied that the effects they produce tend to neutralise each other, so far as our present purpose is concerned. We are thus enabled to investigate the problem as if the stars were at rest, when a sufficiently large number of them are considered together.

Supposing the solar system to be bound on a journey through the celestial spaces, it is obvious that in the course of time apparent displacements would be thereby produced in the relative positions of the stars. The nature of the effects produced may be seen from the following illustration, which has often been given before, but may serve us once again. Let us think of a harbour, the entrance of which is marked by two lights, one on either hand. As the ship approaches the harbour the two lights, which, while the vessel was still a long way off, seemed close together, begin to open out. As the vessel approaches still nearer the lights spread wider and wider until at last, just as the ship enters the port, the two lights have opened so completely that one is on the right hand and the other on the left. In this manner we become familiar with the conception that the lights seem to spread away from the point towards which the motion

is directed. In like manner it is easy to see that when a vessel is sailing away from the port, the beacons seem gradually to draw in together. This consideration provides us with the means of discovering the point in the sky towards which the motion of the sun is directed. We expect that point to be indicated by the circumstance that the stars appear to be spreading away from it.

The correctness of the inference that this is the spot towards which the system is moving will be confirmed, if at the same time it be noticed that the stars are drawing in towards that point in the celestial sphere which lies diametrically opposite. This investigation has been conducted with extreme care. Many astronomers, beginning with William Herschel, have applied themselves to its solution. The most complete investigation is that recently undertaken by Herr Stumpe. He has employed a very large number of stars, and he has adopted every precaution to insure accuracy in the results. Means are provided to enable the precision of his determinations to be properly tested. Stumpe has divided the stars used in his inquiry into four different groups, and he has obtained an independent determination from each of these groups. It is naturally of much interest to inquire where the point in the heavens is situated towards which, at the present time, the solar system appears to be wending its way. Each of the four groups which Stumpe employed has given him a distinct determination. The several investigations agree in locating the point within the limits of the constellation Lyra, adjoining that region of Hercules in which the earlier and less complete investigation of the same problem had located the apex of the sun's way. One of the four points lies actually at the wonderful double-double Epsilon

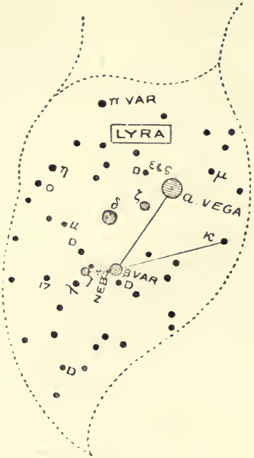

Fig. 1.—The Constellation Lyra.

The star δ marks the point in the heavens towards which the sun's motion is directed.

Lyræ, quite close to the brilliant star Vega. It is therefore interesting to notice that this gem of the Northern skies is also in the vicinity of the actual spot to which the system must be tending. Each of the other three groups of stars employed in Stumpe's researches places the apex at some little distance, though still within the boundaries of the same constellation. We shall certainly not be far wrong if we adopt the average position indicated by these four sets of stars as the final result of this most elaborate inquiry. This point lies close to the star Delta Lyræ, as shown in the above figure (Fig. 1). Perhaps it may be of interest to know how this particular

point may be identified. It can be speedily picked up in the heavens, as follows: Every owner of a telescope is acquainted with Beta Cygni, the most glorious coloured double star that the northern heavens have to offer. A line from Vega to Beta Cygni shows at about one-fourth of the way a bright star, which is Delta Lyræ. It is towards this particular spot of the heavens that the sun, bearing the earth and all the other planets with it, is hurrying at this moment.

The sweep of the solar system through space is represented in the adjoining figure (Fig. 2). The sketch may serve to illustrate the principles on which the determination of the solar velocity is based, but of course it is out of the question that the proper proportions could be observed in such a diagram. In the course of a century the advance of the system towards Lyra will make the stars appear to move in the manner represented by the arrows affixed thereto. Two of the stars are thus seen to spread away from Lyra, while the positions of the other stars are such that they seem to draw in towards the opposite point of the celestial sphere, which is sufficiently indicated by the star Pi Puppis.

If the region to which the motion of our system is directed be adorned by the splendour of Lyra, it is noteworthy that the opposite part of the sphere from which we seem to be flying is also remarkable for its stellar glories. It lies almost midway between Sirius and Canopus.

We shall now turn to the investigation of the allied problem as to the velocity with which the solar system wends its way. A different method of studying the subject which has lately come into practical application

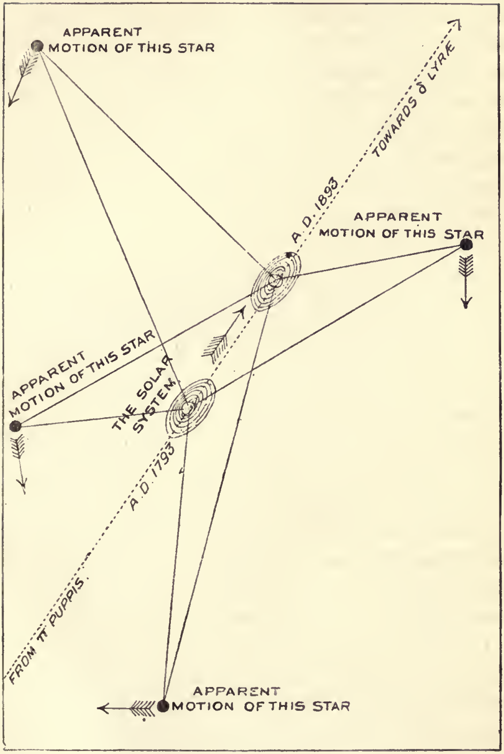

Fig. 2.—The Voyage of the Solar System.

is here to be introduced. It may be remarked that the determination of the speed of the system requires quite a different class of information from that which suffices to

give the situation of the apex of the sun's way. No doubt if we knew the distance of the stars we could then deduce the rate of the solar motion from observation of the apparent displacements of the stars. Unfortunately we are so ignorant of the distances of the great majority of the stars that the process indicated is almost wholly inapplicable with any satisfactory results. It is therefore fortunate that we have a method of investigating the problem which does not require any knowledge as to the stellar distances.

In other parts of this volume we shall dwell upon the important information which the spectroscope has been made to yield with respect to the movement of celestial bodies along the line of vision. It is of the essence of this method of research that the determination of the velocity of the body is obtained quite independently of the star's distance from the observer. The indications of the spectroscope assure us that a star is moving with a velocity of, say, five miles, or ten miles, or some other pace, per second towards the observer or from the observer, and this information has no connection whatever with the remoteness of the star. On this account it becomes specially applicable to the problem we have now to consider.

Of course it will be understood that the stars are themselves endowed with absolute movements. It is therefore not always correct to assume that when the solar system and a star are lessening their distances this is to be attributed entirely or even largely to the movement of the solar system. Both the solar system and the star are moving, and the motion observed is the resultant of the two. If, therefore, we had only a few stars wherewith to control the inquiry, the spectroscopic method could add

no information with regard to the movement of the solar system. But when there is so large a number of stars distributed in such a way that we are entitled to assume that on the whole there is about as much motion in one direction as there is in exactly the opposite direction, then it becomes possible to eliminate the disturbing effects introduced by the fact that the stars are not themselves at rest. When we employ stars enough, we may proceed in the inquiry exactly as if the stars were individually at rest, and as if all the motion perceived coùld be attributed to the movement of the solar system. No doubt there would be a fallacy in this proceeding if it happened that there was a general drift of the stellar motions in one direction. If this was the case, then the methods now employed would attribute, and would falsely attribute, a movement to the solar system equal and opposite to that with which the sidereal system was animated. It is, however, found that by a judicious selection of the stars, we are able to preclude the possibility of such a consentaneous sidereal movement as is here contemplated.

It is obvious that a movement of the solar system towards one region in the heavens must be accompanied by a general diminution of the distances between the stars in that region and our solar system. On the other hand, the distances from the solar system to the stars at the opposite region of the celestial sphere will be generally increased by such a movement. If the stars were at rest, or if a sufficient number of stars were taken to eliminate the consequences of their individual motions, then the spectroscopic search for the movements of the solar system becomes very simple. It is only necessary to measure the rate at which the solar system alters its

distance from the various points around. That point on the celestial sphere where the star shows the velocity of approach to be greatest, can only be the point towards which the solar system is directing its motion. The opposite point of the heavens is that from which the movement of retreat is greatest. At intermediate points there will, throughout one hemisphere, be indications of approach, and throughout the other indications of retreat.

The stars employed in these researches are those which have been investigated spectroscopically by Professor Vogel in his memorable Potsdam observations. The number of the stars made use of was 51. It must be observed that though the method under consideration is admirably adapted to discover the speed of the solar system, it is not equally fitted to indicate the position of the point on the heavens towards which the sun's motion is directed. It can be shown that the conditions are such as to render the detection of that position by the use of the spectroscope much inferior to the determinations afforded by the former process employed by Stumpe. I therefore only use the spectroscopic method for the determination of the velocity deduced on the supposition that the apex of the sun's way is indistinguishable from the position of Delta Lyræ. The result of Vogel's investigation is to show that the velocity of the solar system is about eight miles a second. The accuracy possessed by this result is as usual best indicated by its probable error. It can be shown that the probable error of the statement, that the solar velocity is eight miles per second, is about two, that is to say, it is equally probable that the error of the result lies below two miles a second as that it lies above.

In conclusion it would seem that the sun and the whole solar system are bound on a voyage to that part of the sky which is marked by the star Delta Lyræ. It also appears that the speed with which this motion is urged is such as to bring us every day about 700,000 miles nearer to this part of the sky. In one year the solar system accomplishes a journey of no less than 250,000,000,000 miles. As you look at Delta Lyræ to-night you may reflect that within the last twenty-four hours you have travelled towards it through a distance of nearly three-quarters of a million of miles. So great are the stellar distances, that a period of not less than 180,000 years would be required before our system, even moving at this impetuous speed, could traverse a distance equal to that by which we are separated from the nearest of the stars.

CHAPTER II.

THE PHYSICAL CONDITION OF OTHER WORLDS.

BY the term *world* we mean, usually speaking, this globe on which we stand; but the merest glance at the sky through a telescope will show us that our world is only one of many worlds. Further reflection and study of other parts of the universe will convince us that among these other worlds there are many in different stages, so to speak, of their development. We may represent our earth, for instance, as a world in the maturity of its being, but there are others which exhibit different phases of progress. Some will appear as worlds which are to be regarded in extreme old age, while others again seem to be in an imperfect or immature condition.

Suppose that you came into a room and found a pitcher of water on the table. You placed your hand on the pitcher and you felt that the water was tepid. If you knew that the pitcher had stood there for an hour, you would be able to draw the conclusion that the water must

have been hotter when it was placed there than when you felt it. If when you had felt the temperature you found the water as cold as the air in the room, then you could not infer its original temperature. It might have been a jug of cold water to begin with, or it might have been warm, and have grown cold; you could not tell which. If, however, the water be in the slightest degree warmer than the air in the room, then the argument that it must have cooled from a higher temperature is irresistible. This is so obvious a doctrine that it may seem unnecessary to write it down. But, obvious though it be, it will yet teach us much about the past history, both of our earth and of other globes; especially will it prove instructive about those unfinished worlds of which we are now speaking.

If in a blacksmith's forge you incautiously placed your hand upon a piece of iron, and it burned your fingers, and if the blacksmith told you that the iron had lain there for half an hour, you would not doubt that it must have been much hotter when the blacksmith drew it from the fire than you found it to be. Probably it was even red hot at the time it was laid aside. The argument would still apply if the object, instead of being a lump of iron, were a block of stone; it would apply if the body were as big as a mountain or as big as the moon; neither the fire nor the material would really affect the reasoning. If you found the body to be hot you may feel perfectly certain that hours ago, or days ago, or years ago, or centuries ago, it must have been hotter still. We must apply this argument to that immense globe, 8,000 miles in diameter, on which we are standing. It has an exterior crust of rocks and stones,

and contains a good deal of iron inside. This great ball is undoubtedly very hot in its interior. We have many reasons for knowing this to be the case. The eruptions of volcanoes afford the simplest proof. The smoke, the ashes, and the molten lava which volcanoes pour forth show us that the earth is anything but cool in the lower regions. Other terrestrial phenomena bear similar testimony. Hot springs, for instance, evince the heated condition of the deep-seated rocks. Any miner will tell you that the deeper his mine the hotter he finds his work to be. The gain in heat arises from the fact that the deeper the mine the nearer it lies to the central incandescence. We are not now referring to such heat as is produced by combustion. We are discussing the way in which a body that has once been heated by a fire, or by some other agency, gradually parts with its heat and falls in temperature. There is no means of replenishing to any large extent the heat of the inside of our earth by combustion. The earth's interior temperature must, therefore, on the whole, be simply falling in accordance with the laws of cooling.

The conclusion to which we are led by this reasoning is a remarkable one. We know that our earth has been in existence for an incalculable period of time. We are not even able to estimate how many thousands of years have elapsed since man began his course on our globe, but the human period is merely the latest of all the great periods in world-history. Long before man commenced to live here the earth was the abode of life; countless races of animals, large and small, now generally extinct, roamed through forests of trees, or through vegetation of a kind largely if not wholly different from anything

Fig. 3.—Spiral form of the Nebula in Canes Venatici (Lord Rosse).

which now grows. Time after time have these races of organized beings passed away and been replaced by others entirely different. There were times when this globe was inhabited by reptiles far larger than any terrestrial animals now living. All the zoological gardens in the world at present would not be nearly large enough to contain representative specimens of all the varieties of animals which have from time to time found a home on this earth. But these animals have now passed away, and the only means we have of learning that they ever existed is afforded by the occasional skeletons which in the form of fossils are now and then extracted from the rocks. No one has succeeded in making any reliable estimate of the number of years which have run their course since first this globe assumed its present shape. But no one can doubt that these years are to be reckoned in their millions, though whether these millions are to be expressed in units or tens or hundreds, or in periods even greater still, is a matter beyond our knowledge.

During all these ages the earth must gradually have been growing colder, and therefore at the beginning it must have been hotter than it is to-day. As we look back earlier and still earlier the earth ever seems hotter and hotter through the ages. At least the argument points to a time when the earth must have been hot even to its surface, so hot that you could not stand on it, and then earlier still it was red hot, white hot, and molten. Even here the argument does not fail; we find the heat must have been greater and greater the further we look back, until at last we come to a time when the now solid materials of our earth must have been in a widely different form. For we know that the most infusible materials

like steel or flint can, if they be heated sufficiently high, not only be transformed into a liquid, but even be driven off into a vapour. Thus we learn that there was a time when our earth was merely a mass of glowing gas.

A great deal of light is thrown upon this subject by looking at other worlds, some of which are to be seen in quite an unfinished state even at the present moment. They are unfinished in the sense that the gaseous material has not yet condensed down sufficiently to form a solid globe. There are thousands of bodies with which astronomers are acquainted which will in one way or another illustrate these phases of our earth's past history. I shall only mention one, which is typical of a remarkable class of similar objects. Fig. 3 represents one of the famous spiral nebulæ, discovered many years ago by the late Earl of Rosse. The object is invisible to the naked eye. It seems like a haze surrounding the stars, which the telescope discloses in considerable numbers, as shown in the picture. When viewed through an instrument of sufficient power a marvellous spectacle is revealed. There are wisps and patches of glowing cloud-like material which shine not as our clouds do by reflecting to us the sunlight. This celestial cloud is self-luminous; it is in fact composed of vapours so intensely heated that they glow with fervour. As I write, I have Lord Rosse's elaborate drawing of this nebula before me, and on the margin of this stupendous object the nebula fades away so tenderly that it is almost impossible to say where the luminosity terminates. Probably this nebula will in some remote age gradually condense down into more solid substances. It contains, no doubt, enough material to make many globes as big as our earth. Before, however, it

settles down into dark bodies like the earth, it will have to pass through stages in which its condensing materials will form bright sun-like bodies. It seems as if this

Fig. 4.—Crab Nebula.

process of condensation might almost be witnessed at the present time in some parts of the great object.

There are also some very striking nebulæ which are often spoken of as *planetary.* They are literally balls of bluish-coloured gas or vapour, apparently more dense

than that which forms the nebula now under consideration. Such globes are, doubtless, undergoing condensation, and may be regarded as incipient worlds.

Our fellow-planets like the earth are guided and held in their ever-circling way by the attraction of the sun, while they are also illuminated by the light which he pours forth with such liberality, and warmed with the rays of heat which he sends them. The sun does not, indeed, confer these benefits in an equal degree on all the members of his family; those which are nearer to him get much, perhaps too much according to our notions, of his heat, while those like Uranus or Neptune, which lie on the outskirts of the system, get little, perhaps too little, of those particular benefits which he dispenses. This world of ours thus occupies a somewhat intermediate position. The structure of the human body would have to be considerably modified if we were to find a congenial residence either so near the sun as Mercury or so far from him as Neptune. As we live on this earth in the temperate regions, and suffer neither from the fearful heat at the equator nor from the horrors of the frozen poles, so too does our entire world enjoy what we may describe as a temperate situation in the series of bodies belonging to the solar system.

In other respects, too, our position is an intermediate one. There are some planets, such as Mars and Mercury, which are very much smaller than our earth. There are other planets, such as Jupiter and Saturn, which are enormously greater than the earth; and there is Venus, our beautiful neighbour, which is almost exactly the same size. Considering that this earth may be taken as an average specimen of the worlds which form the sun's

family, it is natural to inquire how far the other planets may be constituted in the same way as our own. Most of the questions which we should like to ask on this head are such as, unhappily, cannot be answered. Especially should we like to know whether the other planets are inhabited, but on this our greatest telescopes can give us no information whatever, and we can only form the vaguest surmises. The features that would be discernible on the neighbouring planets must be immense indeed. It would, for example, be utterly impossible for us to recognise towns, even if such objects as towns existed, though it might still be possible to discern the broader outlines of extensive continents or mighty oceans. We could also observe the clouds, if clouds existed, around a neighbouring planet, because owing to their extent and to their position on the outside they would be comparatively easy to see, while the incessant changes of the clouds would render them an attractive feature to the observer.

We have accordingly in this chapter decided to say what we can with respect to the clouds and oceans which are to be met with on some of the other planets. Even here, however, we must be content with a knowledge which is much more scanty than an intelligent curiosity would desire. In many of the planets we can see little or nothing of this kind that can be certainly made out, while even on those which we can see best it is only the very broadest and most striking features that can be discerned. Of Venus, unhappily, we can see nothing or next to nothing that would give us any information as to the presence or the absence of oceans and clouds. The loveliness of the evening star is due to the brilliancy of the sun-beams in which she is decked,

but this very brilliancy is inconvenient in the telescope, where it merely appears as a glare which renders all details invisible. Beyond a few ill-defined and incon-

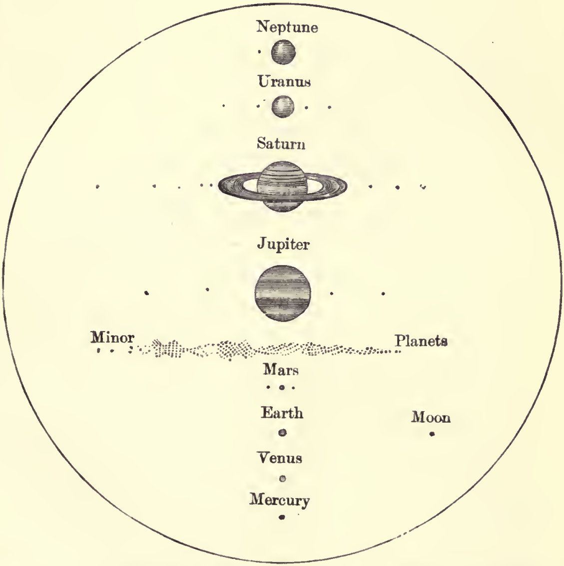

Fig. 5.—Comparative Sizes of the Planets. The great circle represents the Sun.

spicuous marks, nothing has ever been seen on this planet which is of any interest for our present purpose.

Though clouds are comparatively an unimportant feature on Mars, they assume the most extraordinary importance on Jupiter. He is a gigantic body, by far

the largest of all the planets; larger, indeed, than all the other planets put together. Viewed in the telescope, the surface of Jupiter is usually seen crossed by two belts, one above and one below his equator. You would produce something like them on any ordinary globe by making a broad belt on the tropic of Cancer, and another on the tropic of Capricorn. Now, these belts on Jupiter are not fixed features of his surface; they are constantly changing their aspect. Sometimes they are hardly to be seen at all, and on other occasions they widen their limits and become irregular at their edges; the greater part of the surface of the planet is more or less covered over with similar markings. We see nothing on this great planet that resembles the oceans and continents on Mars, nor have we any indications of arctic regions on Jupiter's surface. In fact, the longer we look at Jupiter, the more we become convinced that the surface of the planet is swathed with a mighty volume of clouds so dense and so impenetrable that our most powerful telescopes have never yet been able to pierce through them down to the solid surface of the planet. Indeed we can hardly say whether this planet has any solid interior at all. There is one object on Jupiter known as "the great red spot," which for several years was more or less recognisable. This seemed to be a great volcano, or some other projection from beneath, which was tall enough and large enough to make itself visible through the mighty covering of clouds which act as an effectual screen to hide all objects of lower prominence.

There is another very interesting way in which we can confirm the fact that the apparent volume of Jupiter is swollen by these mighty clouds which so closely encase

him. Careful measurements having been made it has been shown that Jupiter is 1,200 times bigger than our earth: in other words, that 1,200 globes, each as large as this earth, rolled together into one, would only form a ball as big as this mighty planet. Astronomers also have the means of weighing a great planet as well as of measuring it. How this weighing is to be effected I shall not here pause to describe; suffice it to say that the little moons by which Jupiter is attended afford by their movements the means of answering the question; and the answer is a significant one, for we find that Jupiter is about 300 times as heavy as the earth. This gives us, indeed, an impressive idea of the magnificence of the mightiest of the planets. Were a pair of gigantic weighing scales constructed, and Jupiter placed in one of these scales, then it would require 300 globes each as heavy as the earth to be placed in the other before the mighty balance could turn. Yet when we remember that Jupiter is 1,200 times as large as the earth we may well feel surprised at learning that he is only 300 times as heavy. Were the constitution of the planet at all like that of our earth, then the weights and the sizes should observe the same proportions, just as one solid iron ball, when ten times as big as another, will be ten times as heavy. The lightness of Jupiter in comparison with his size is really the point that merits our astonishment. He is, indeed, not so very much heavier than a globe of water the same size would be, while our earth is five times as heavy as a globe of water equally large. The true explanation is that Jupiter is so swollen by these enormous masses of cloud which surround him as to give him a bigness altogether out of proportion to his mass. Therefore, as Mars gives

us an illustration of the existence of oceans on other planets, so Jupiter provides us with a splendid example of a planet encompassed with clouds.

It is interesting to compare the circumstances attending our residence on this earth with the corresponding conditions that would be found if we could change our abode from this globe to another planet. I propose to discuss a few of the points which arise when we consider such questions. In the first place we must remember that our bodies have been specially organized and adapted to suit our surroundings on this particular world. I do not think it is at all probable that a man could exist, even for five minutes, on any other planet or any other body in the universe. We know that within even the limits of our own earth, each one of us has to be provided with a constitution appropriate to a particular climate. An Eskimo is suitably placed in the arctic regions, a negro on the equator; and were they to change places, it is hard to say whether the heat would not have killed the Eskimo even before the cold killed the negro. But such an attempt at acclimatization would be easy when compared with that which would be required before an inhabitant adapted to one globe could accommodate himself to a residence on another. Indeed, there seem to be innumerable difficulties in supposing that there can be any residence for man, or for any beings nearly resembling man, elsewhere than on his own earth.

Let us specially review a few of the other globes, beginning with the sun. I think we need not give many reasons to show that a man could not live there long. Every boy knows how a burning glass can kindle a piece of paper by concentrating the sun's rays. Some great

burning glasses have been constructed with which iron, steel, and even flints have been actually melted by the sun's heat. It can be proved that the sun himself must be hotter than any temperature that can be produced in the focus of the most powerful burning glass. We certainly cannot conceive any organized being which would

Fig. 6.—The Clouds on Jupiter, October 14th, 1891.

find a congenial residence in a temperature vastly hotter than that of the most powerful furnace that has ever been known. Assuredly there can be no life on the sun.

The next celestial world in importance to the sun is, of course, the moon. Could we find here an eligible abode for mankind? The moon would, no doubt, provide the necessary alternation from day to night, but the day on

the moon would last for a fortnight, and then there would be black night for another fortnight. During the long day the moon would be terribly scorched, a circumstance which would be hardly compensated for by the fact that even if we survived the scorching we should certainly be frozen to death during the ensuing night. But there would be

Fig. 7.—Jupiter, showing the shadow of a Satellite, October 15th, 1891.

other insuperable difficulties attending an attempt to make an abode on the moon. The absence of water is one of them, while a still more immediate trouble would arise from the deficiency, if not total absence, of air suitable for respiration. Indeed, it is almost impossible for us to conceive what an airless world would be like. Fishes out of water would not be more uncomfortable than we should

find ourselves. But suppose that we managed to bring a supply of oxygen that might enable us to avoid suffocation by the use of artificial respiration, we should still find the moon a very strange world. We could hear nothing, for sound exists not except in air. We could strike no match or light no fire. We could feel no wind and see no clouds. There would be also an embarrassment of a different kind which there would not be any way of obviating.

Suppose that we were actually on the moon, and that we had in some way obtained the necessary provision both of air and of water, and had begun to walk about, we should experience sensations of a novel description. The extraordinary lightness of everything would be specially noticeable. Take a lump of iron which weighs six pounds on the earth, you would find on the moon that it seemed to weigh only as much as one pound would do on the earth. Everybody knows that it requires considerable exertion to lift a 56 lbs. weight here, but on the moon it would hardly require as much effort as you ordinarily have to put forth to lift ten pounds. Indeed, the weight of every object on the moon would be reduced to the sixth part of that which the same object has on the earth. No doubt in some ways this might prove a convenience to the moon dwellers. Their bodies would partake of the general buoyancy; walking and running would be amazingly facilitated; and the same effort that would enable you to jump over an obstacle three feet high here would carry you with ease over a wall eighteen feet high on the moon. A good cricketer can throw a ball about a hundred yards here. If he made the same exertion on the moon he could throw the ball over a third of

a mile. The diminished gravitation would prove of service in athletic performances on the moon. Not only would a bicycle be driven along with unparalleled ease and rapidity if the lunar roads were smooth, but even the disagreeable process of taking a header over the handles would lose its terrors, for the lunar bicyclist would fall gently and softly to his mother earth. It may, however, be questioned whether our bodies would be adapted for a life under such conditions. It seems almost certain that as the muscular system of the human body has been arranged to work with the particular gravitation that is found on this earth, it would be impossible for it to be accommodated to a gravitation which had only a sixth of the intensity for which it was adapted. On these grounds we conclude that neither the times nor the seasons, neither the gravitation nor the other distinctive features of the moon, would permit it to be an endurable abode for life of the types we are acquainted with.

Let us now consider some of the more distant worlds, and examine their claims to be regarded as possible homes for beings in any degree resembling ourselves. There are many of these worlds with regard to which we may at once decide in the negative. Could we, for instance, live on a planet like Neptune? It lies thirty times as far from the sun as we do. The share of the light and heat from the sun which a Neptunian inhabitant would receive could only be the nine-hundredth part of that which is dispensed to every dweller on this earth. This fact alone would seem to show an insuperable obstacle to the existence of any life on Neptune resembling those types of life with which we are familiar. The orbit of

Neptune is also so vast that the planet requires a period of 165 years in order to complete a single revolution. The changes of Neptunian seasons must, therefore, be extremely protracted. A man who was born at mid-winter in Neptune would have reached extreme old age if he survived until the next ensuing mid-summer.

I cannot discuss the times and seasons of all the celestial bodies, so I have taken a few typical instances. Neptune was appropriate as being the most remote planet. Now let us speak of Jupiter, the greatest planet. The day and night on Jupiter are both extremely short, for together they do not quite amount to ten hours. Jupiter's year, however, is almost twelve of our years. Although a man on Jupiter would only receive one-twenty-fifth part of the heat of the sun that he would do on the earth, yet it does not seem likely that there would be reason to apprehend that Jupiter would be uninhabitable from cold. Quite the contrary is the case. Indeed, it seems not unlikely that the excessive heat of Jupiter would be found intolerable by beings with nerves like ours. This heat has, however, not come from the sun; it is the internal heat of the planet itself, which has not yet sufficiently cooled down from that original fiery condition characteristic of every body of our system in its initial stages.

Jupiter certainly has an atmosphere, but we do not know from what gases that atmosphere may have been blended. It might consist of materials noxious, if not actually poisonous; and in any case it is extremely unlikely that it should contain both the ingredients and the proportions suited to our organs of respiration. But there are independent grounds for knowing that Jupiter must be an impossible home for beings so constituted as we are. On

the moon every object would be deprived of five-sixths of its weight, because the moon is a comparatively small globe. Were we, however, to be transferred to Jupiter, the weight of every object would receive an extraordinary augmentation. Our muscles would be found utterly inadequate to their work. Walking, or even standing, would involve the most fearful exertion, while rising from bed in the morning would be a difficult, indeed, probably, an impossible process. I see no likelihood that Jupiter can be the home of any life whatever.

We may dismiss from our present consideration such bodies as the comets. A comet moves during the greater part of its course through the depths of space at inconceivable distances from the sun. Out there, the comet traverses regions where the cold would be absolutely incompatible with life of any type conceivable by us. Then for a brief period, to be measured in months, weeks, days, or even hours, the comet is wheeling around the sun, where it is often exposed to a frightful temperature sufficient to fuse and even vapourise bars of wrought iron. A comet, indeed, is not a likely abode for life, though I ought to mention that comets often contain the element carbon. This is a very singular fact when it is remembered that carbon is one of the substances essentially associated with life in the forms in which we know it.

There is, however, one body in our system whose times and whose seasons accord so closely with our own that it is impossible not to believe that life of some kind may there be found. The length of the day and night together on Mars is 24 hours 37 minutes; that is practically only about half an hour greater than the corresponding period for our own globe. The year of Mars is, no doubt

longer than ours, being about a year and eleven months. The size of Mars is less than the size of our earth, and, therefore, the gravitation on Mars is not so great as we have here. I do not mean to say that it is the least likely that any man, woman, or child transplanted from this earth to Mars could live and thrive there. The temperature might be endurable, and water appears to be not wanting, but I do not think we have any reason to expect that the atmosphere would suit human beings either in quantity or quality. As, however, the case of Mars will be discussed in another chapter in this volume, we do not here refer to it further.

CHAPTER III.

THE WANDERINGS OF THE NORTH POLE.

ON a visit to Cambridge, Professor E. E. Barnard, the discoverer of the fifth satellite of Jupiter, exhibited at the Cavendish Laboratory a most interesting collection of photographs made at the Lick Observatory. These pictures were obtained by a six-inch photographic lens of three-feet focus, attached to an ordinary equatorial, the telescope of which was used as a guider when it was desired to obtain a picture of the stars with a long exposure. Among the advantages of this process may be reckoned the large field that is thereby obtained, many of the plates that he exhibited containing as much as sixteen square degrees. I am, however, not now going to speak of Barnard's marvellous views of the Milky Way, nor of the plate on which a comet was discovered, nor of the vicissitudes of Holme's comet, nor of that wonderful picture in which Swift's comet actually appears to be producing, by a process of gemmation, an offshoot which is evidently adapted for

an independent cometary existence. The picture to which I wish specially to refer in connection with our subject was obtained when the instrument was directed towards the North Celestial Pole.

In this particular case the clockwork which is ordinarily employed to keep the stars acting at the same point of the plate was dispensed with. The telescope, in fact, remained fixed while the heavens rotated in obedience to the diurnal motion. Under these circumstances each star, as minute after minute passed by, produced an image on a different part of the plate; the consequence of which was that, when the picture was developed, the record which the star was found to have left was a long trail instead of a sharply defined point. As each star appeared to describe a circle in the sky around the Pole, and as, in the vicinity of the Pole, these circles were small enough to be included in the plate, this polar photograph exhibited a striking spectacle. It displayed a large number of concentric circles, or rather, I should say, of portions of circles, for the exposures having lasted for about four hours, about one-sixth of each circumference was completed during that time. The effect thus produced was that of a number of circular arcs of varying sizes, and of different degrees of brightness. A representation of this photograph is given in Fig. 8.

Most conspicuous was the trail produced by the actual Pole Star itself. It is well known, of course, that though the situation of the Pole is conveniently marked by the fortunate circumstance that a bright star happened during the present century to lie in its immediate vicinity, yet, of course, this star is not actually at the Pole, and consequently, like all the other stars, Polaris itself must be re-

volving in a circle whereof the centre lies at the true Pole. The brighter the star the brighter is the trail which it produces, so that the circle made by Polaris is much more conspicuous than the circles produced by the other stars

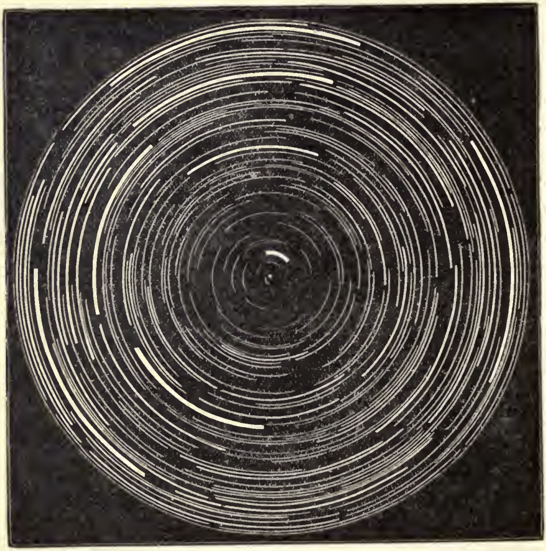

Fig. 8.—The Photograph of the Stars in their Diurnal Motion round the Pole.

of inferior lustre. It is, however, to be noted that some of the faint stars lie much closer to the Pole than Polaris itself. There is, indeed, one very minute object so close to the Pole that the circle in which its movements are performed seems very little more than a point when

represented on the screen on which the slide was projected.

The interesting circumstance was noted that there appeared to be occasional interruptions to the continuity of the circular arcs. This was due to the fact that clouds had interposed during the intervals represented by the interruptions. A practical application is thus suggested, which has been made to render useful service at Harvard College Observatory. Every night, and all night long, a plate is there exposed to this particular part of the sky, and the degree in which the Pole Star leaves a more or less complete trail affords an indication of the clearness or cloudiness of the sky throughout the course of the night. From the positions of the parts where the trail has been interrupted it is possible not only to learn the amount of cloudiness that has prevailed, but the particular hours during which it has lasted.

This interesting system of concentric polar circles affords us perhaps the most striking visual representation that could possibly be obtained of the existence of that point in the heavens which we know as the Pole. The picture thus exhibited was a graphic illustration of the Copernican doctrine that the diurnal stellar movement was indeed only apparent, being, of course, due to the rotation of the earth on its axis.

Suppose that a photograph, like that which I have been describing, were to be taken at intervals of a century, it would be found that the centre of the system of circles, that is to say, the veritable Pole itself, was gradually changing on the heavens. I do not by this mean that the stars themselves would be found to have shifted their

places relatively to each other. No doubt there is some effect of this kind, but it is an insignificant one, and need not at present concern us. The essential point to be noticed is, that the stars which happen to lie in the vicinity of the Pole would have a changed relation to the Pole in consequence of the fact that this latter point is itself in incessant movement. At the present time the Pole is advancing in such a direction that it is getting nearer to the Pole Star, so that the actual circle which the Pole Star is describing is becoming less and less. The time will come when the circle which this star performs will have reached its lowest dimensions, but the Pole will still be moving on its way, and then, of course, the dimensions of the circle traversed by the Pole Star will undergo a corresponding increase. As hundreds of years, and thousands of years roll by the Pole will retreat further and further from the Pole Star, so that in the course of a period as far in the future as the foundation of Rome was far in the past, the Pole will be no longer sufficiently near the Pole Star to enable the latter to render to astronomers the peculiar services which it does at present.

Looking still further ahead, we find that in the course of about twelve thousand years the Pole will have gained a position as remote as it possibly can from that position which it now occupies. This most critical point in the heavens will then lie not far from the star Vega, the brightest point in the northern sky, and then it will commence to return, so that after the lapse of about twenty-five thousand years the Pole will be found again in the same celestial neighbourhood in which it is to-night, having, in the meantime, traversed a mighty circle through

the constellations. In all this there is no novelty; these movements of the Pole are so conspicuous that they were detected long before the introduction of accurate instruments. They were discovered so far back as the time of Hipparchus, and the exposition of their cause by Newton was one of the triumphs of his doctrine of universal gravitation.

In giving the title of "The Wanderings of the North Pole" to this chapter I did not, however, intend to discuss the movements to which I have hitherto referred. They are so familiar that every astronomer has to attend to them practically in the reduction of almost every observation of the place of a celestial body. It was, however, necessary to make the reference which I have done to this subject in order that the argument on which we are presently to enter should be made sufficiently clear. It must be noted that the expression, "the North Pole," is ambiguous. It may mean either of two things, which are quite distinct. In the case we have already spoken of, I understand by the North Pole that point on the celestial sphere which is the centre of the system of concentric circles described by the circumpolar stars. The other sense in which the North Pole is used is the terrestrial one; it denotes that point on this earth which has been the goal of so many expeditions, and to reach which has been the ambition of so many illustrious navigators.

We have a general notion that the terrestrial North Pole lies in a desolate region of eternal ice, somewhat relieved by the circumstance that, for six months of the year, the frozen prospect is brightened by perpetual day, though on the other hand, during the remaining six months of the year this region is the abode of perpetual night. The

North Pole is that hitherto unattainable point on our globe on which, if an observer could take his station, he would find that the phenomena of the rising and the setting of the stars, so familiar elsewhere, was non-existent. Each star viewed from the coign of vantage offered by the North Pole would move round and round in a horizontal circle; and the system of concentric circles would be directly overhead. In midsummer the sun would seem to revolve around, remaining practically at the same elevation above the horizon for a few days, until it slowly began to wend its way downwards in a spiral. In a couple of months it would draw near the horizon, and as day after day passed by the luminary would descend lower and lower until its edge grazed the horizon all round. The setting of the sun for the long winter would then be about to commence, and gradually less and less of the disc would remain perceptible. Finally the sun would disappear altogether, though for many days afterwards a twilight glow would travel round the whole hemisphere, ever getting less and less, until at last all indications of the sun had vanished.

The utter darkness of winter would then ensue for months, mitigated only so far as celestial luminaries were concerned by starlight or occasional moonlight. Doubtless, however, the fitful gleams of the aurora would often suffice to render the surrounding desolation visible. Then as the spring drew near, if, indeed, such a word as spring be at all applicable to an abode of utter dreariness, a faint twilight would be just discernible. The illuminated portion of the sky would move round and round the horizon each twenty-four hours, gradually becoming more and more conspicuous, until at last the edge of the sun appeared. Then, by a spiral movement inverse to that with which

its descent was accomplished, the great luminary would steal above the horizon, there to continue for a period of six months until the commencement of the ensuing winter. Indeed, the actual duration of apparent summer would be somewhat protracted in consequence of the effect of refraction in raising the sun visually above the horizon when in reality it was still below. The result would be to lengthen the summer at one end and to anticipate it at the other. Such would be the astronomical conditions at the North Pole; that anomalous point, from which every other locality on the globe lies due south, that mysterious point which up to the present never seems to have been approached by man within a distance less than 400 miles, unless, indeed, as is not improbably the case, the Preglacial Man who lived in the last genial period found a temperate climate and enjoyable conditions even at the latitude of 90°.

For our present purpose it will be necessary to get a very clear idea as to the precise point on the earth which we mean when we speak of the North Pole. As our knowledge of it is almost entirely derived from astronomical phenomena it is necessary to assign the exact locality of the Pole by a strict definition depending on astronomical facts. Supposing that Nansen does succeed in his expedition, as every one hopes that he will, and does penetrate within that circle of 400 miles' radius where the foot of civilized man has never yet trod, how is he to identify that particular spot on this globe which is to be defined as the North Pole? It was for this purpose that at the commencement of this paper I referred to that photograph of the concentric circles which illustrated so forcibly the position of the Pole in the heavens.

Imagine that your eye was placed at the centre of the earth, and that you had a long slender tube from that centre to the surface through which you could look out at the celestial sphere; if that tube be placed in such a way that, when looking from the centre of the earth through this tube your vision was directed exactly to that particular point of the heavens which is the centre of the circle now described by the Pole Star and the other circumpolar stars, then that spot in which the end of the tube passes out through the surface of the earth is the North Pole. Imagine a stake to be driven into the earth at the place named, then the position of that stake is the critical spot on our globe which has been the object of so much scientific investigation and of so much maritime enterprise.

The reader must not think that I am attempting to be hyper-accurate in this definition of the North Pole; no doubt, in our ordinary language we often think of the Pole as something synonymous with the polar regions, an ill-defined and vaguely known wilderness of ice. For scientific purposes it is, however, essential to understand that the Pole is a very definitely marked point, and we must assign its position accurately, not merely within miles, but even within feet. Indeed, it is a truly extraordinary circumstance that, considering that no one, with the possible exception just referred to, has ever yet been within so many hundreds of miles of the Pole, we should be able to locate it so precisely that we are absolutely certain of its position to within an area not larger than that covered by a good-sized church.

We have seen that the North Pole in the sky is in incessant movement, and that the journeys which it

accomplishes in the course of many centuries extend over a wide sweep of the heavens; this naturally suggests the question, Does the Pole in the earth move about in the body of the earth in any similar manner, and if so, what is the nature and extent of its variation? Here is the point about which those researches have been made which it is my object to discuss. Let us first see clearly the issue that is raised. At the time of the building of the Pyramids the Pole in the heavens was in quite a different place from its present position; the present Pole Star had not at that time the slightest title to be so called; in fact, the point around which the heavens revolved lay in a wholly different constellation. It was certainly not far from the star Alpha Draconis about 3000 B.C., and we could indicate its position quite definitely if we had any exact knowledge as to the date of the erection of the Pyramids. It is, however, plain that the difference was so patent between the celestial Pole at the time of the Pyramids and the celestial Pole of later centuries, that it could not be overlooked in attentive observation of the heavens. As the North Pole in the sky was, therefore, so different in the time of the Pharaohs from the North Pole in the time of Victoria, it is proper to ask whether there was a like difference, or any difference at all, between the terrestrial Pole at the time of the building of the Pyramids and that terrestrial Pole, in quest of which Nansen has just set off. If Pharaoh had despatched a successful expedition to the North Pole and driven a post in there to mark it, and if Nansen were now successful, would he find that the North Pole in the earth which he was to mark occupied the same position or a different position from that which had been discovered thousands of years previously?

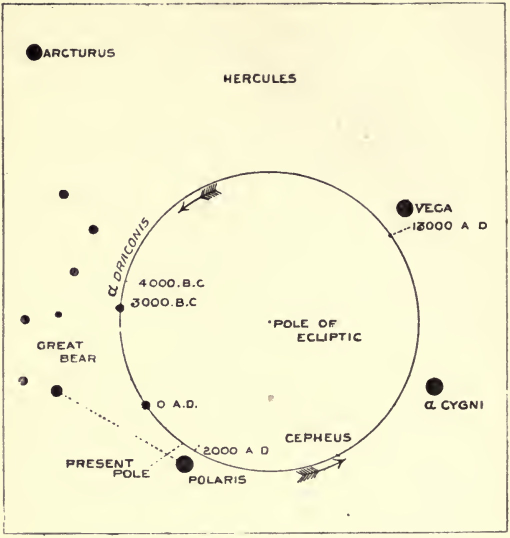

Fig. 9.—The Movement of the Celestial Pole.

At first one might hastily say that there must be such a difference, for it will be remembered that I have defined the North Pole in the earth as that point through which the tube passes which would permit an eye placed at the centre of the earth to view the North Pole in the sky. If, therefore, the North Pole in the sky had undergone a great change in its position, it might seem obvious that the tube from the earth's centre to its surface which would now conduct the vision from that centre to the north celestial Pole would emerge at a different point of the earth's crust from that which it

formerly occupied. We have here to deal with the case that arises not unfrequently in astronomy, in which a fact of broad general truth requires a minute degree of qualification; indeed, it is not too much to say that it is in this qualification of broad general truths that many of the greatest discoveries in physical science have consisted. And such is the case in the present instance. There is a broad general truth and there is the qualification of it. It is the qualification that constitutes the essential discovery which it is my object here to set forth.

Before doing so it will be necessary for me to lay down the broad general truth that the North Pole of the earth as it existed in the time of the Pharaohs appears to be practically the same as the North Pole of the earth now. It seems perfectly certain that at any time within the last 10,000 years the North Pole might have been found within a region on the earth's surface not larger than Hyde Park. Indeed, the limits might be drawn much more closely. It is quite possible that many an edifice in London occupies an area sufficiently great to cover the holes that would be made by all the posts that might be driven to mark the precise sites of the North Pole on the earth not only for the last 5,000 or 10,000 years, but probably for much longer periods. It is very likely that the North Pole at the time of the Glacial Epoch was practically indistinguishable from the North Pole now; in fact, the constancy, or I should, perhaps, rather say, the sensible constancy of the situation of this most critical point in our globe is one of the most astonishing facts in terrestrial physics.

Let us, then, assume this broad general fact of the permanency in the position of the North Pole, and deduce

the obvious consequence it implies with regard to the earth's movement. At this point we find the convenience of the time-honoured illustration in our geography books which likens the earth to an orange. Let us thrust a knitting-needle through the orange along its shortest diameter to represent the axis about which the earth rotates. Not only does the earth perform one revolution about this axis in the space of each sidereal day, but the axis itself has a movement. If the earth's axis always remained fixed, or never had any motion except in a direction parallel to itself, then the point on the sky to which it was directed would never change. We have, however, seen that the Pole in the sky is incessantly altering its position; we are therefore taught that the direction of the earth's axis of rotation is constantly changing. To simulate the movement by the orange and knitting-needle we must imagine the orange to rotate around its axis once in that period of twenty-three hours and fifty-six minutes which is well known as the length of the sidereal day; while at the same time the knitting-needle itself, bearing, of course, the orange with it, performs a conical movement with such extreme slowness that not less than 25,000 years is occupied in making the circuit. The movement, as has often been pointed out, is like that of a peg-top which rotates rapidly on its axis while at the same time the axis itself has a slow revolving motion. Thus the phenomena which are presented in the rotation of the earth demonstrate that the axis about which the earth rotates occupies what is, at all events, approximately a fixed position in the earth, though not a fixed position in space. We can hardly be surprised at this result; it merely implies that the earth acts like a

rigid body on the whole, and does not permit the axis about which it is turning to change its position.

It will now be easily understood how it comes to pass that the position of the North Pole upon the earth has not appreciably changed in the course of thousands of years. The axis around which the earth rotates has retained a permanent position relative to the earth itself; but with

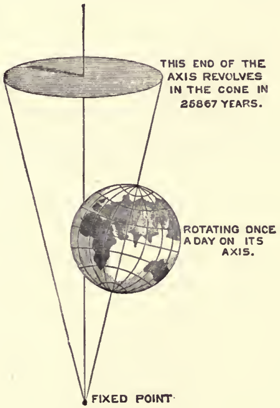

Fig. 10.—Rotation of the Pole.

regard to its direction in space it has continuously changed, it is at this moment changing, and will continue to change. So far our knowledge extended up to within the last few years, but quite recently a closer inquiry has been made into this, as into so many other physical subjects, and the result has been to disclose the important fact that, though the phenomena as just described are very nearly true, they must receive a certain minute

qualification. Complete examination of this subject is desirable, not only on account of its natural importance, but also because it illustrates the refinements of which modern astronomical processes are susceptible.

I have stated that the position of the terrestrial Pole undergoes no large or considerable fluctuation. But while we admit that no large fluctuation is possible, it is proper to consider whether there may not be a small fluctuation. It is certain that the position of the Pole as it would be marked by a post driven into the earth to-day cannot differ by a mile from the position in which the same point would be marked last year or next year. But does it differ at all? Is it absolutely exactly the same? Would there be a difference not indeed of miles but of yards or of feet between the precise position of the Pole on the earth determined at successive intervals of time? Would it be the same if we carried out our comparisons not merely between one year and another, but day after day, week after week, month after month? No doubt the more obvious phenomena proclaim in the most unmistakable manner that the position of the Pole is substantially invariable. If, therefore, there be any fluctuations in its position, these can only be disclosed by careful scrutiny of minute phenomena which are too delicate to be detected by the coarser methods of observation. There is indeed a certain presumption in favour of the notion that absolute constancy in the position of the Pole need not be expected. Almost every statement of astronomical doctrine requires its qualification, and it would seem indeed unlikely that when sufficient refinement was introduced into the measurements the position of the Pole in the earth should appear to be absolutely unalterable.

Until a very recent period the evidence on the subject was almost altogether negative; it was no doubt recognised that there might be some fluctuations in the position of the Pole, but it was known that they could only be extremely small, and it was believed that in all probability those fluctuations must be comprised within those slender limits which are too much affected by inevitable errors of observation to afford any reliable result. Perseverance in this interesting inquiry has at last been rewarded, and as in so many similar cases we are indebted to the labours of many independent workers for the recent extension of our knowledge. We are, however, at present most interested by the labours of Mr. Chandler, a distinguished American astronomer, who has made an exhaustive examination into the subject. The result has been to prove that the Pole does undergo movement in the body of the earth. Mr. Chandler has been so successful as to have determined the law of those polar movements, and he has found that when they are taken into consideration an important improvement in certain delicate astronomical inquiries is the result. These valuable investigations merit, in the highest degree, the attention, not only of those who are specially devoted to astronomical and mathematical researches, but of that large and ever-increasing class who are anxious for general knowledge with regard to the physical phenomena of our globe.

At first sight it might seem difficult indeed to conduct the investigation of this question. Here is a point on the earth's surface, this wonderful North Pole, which, so far as we certainly know, has never yet been approached within 400 miles, and yet we are so solicitous about the

position of this Pole and about its movement that we demand a knowledge of its whereabouts with an accuracy which at first appears wholly unattainable. It sounds almost incredible when we are told that a shift in the position of the North Pole to the extent of twenty yards, or even of twenty feet, is appreciable, notwithstanding that we have never been able to get nearer to it than from one end of England to the other. Indeed, as a matter of fact, our knowledge of the movements of the Pole are derived from observations made not alone hundreds but even many thousands of miles distant. It is in such observatories as those at Greenwich or Berlin, Pulkova or Washington, that the determinations have been made by which changes in the position of the Pole can be ascertained with a delicacy and precision for which those who were not aware of the refinement of modern astronomical methods would hardly be prepared. I do not, however, imply that the observations conducting to the discoveries now about to be considered have been exclusively obtained at the observatories I have named. There is a large number of similar institutions over the globe which have been made to bear their testimony. Tens of thousands of different observations have been brought together, and by discussing them it has been found possible to remove a large part of the errors by which such work is necessarily affected, and to elicit from the vast mass those grains of truth which could not have been discovered had it not been for the enormous amount of material that was available. Mr. Chandler has discussed these matters in a remarkable series of papers, and it will be necessary for me now to enter into some little detail, both as regards the kind of observations that have

been made, and the results to which astronomers have been thereby conducted.

Greenwich Observatory lies more than 2,000 miles from the North Pole, and yet if the Pole were to shift by as much as the width of Regent Street, the fact that it had done so would be quite perceptible at Greenwich. Let me endeavour to explain how such a measurement could be achieved. In finding the latitude at any locality we desire, of course, to know the distance between the locality and the Equator, expressed in angular magnitude. But though this is distinctly the definition of latitude, it does not at once convey the idea as to how this element can be ascertained. How, for instance, would an astronomer at Greenwich be able to learn the angular distance of the observatory from the Equator ? The Equator is not marked on the sky, and it is obvious that the observer must employ a somewhat indirect process to ascertain what he wants. Here, again, we have to invoke the aid of that celestial Pole to which I have so often referred.

Think of that point on the sky which is the common centre of the circles exhibited on Professor Barnard's photograph. That point is not indeed marked by any special star, but it is completely defined by the circumstance that it is the centre of the track performed by the circumpolar stars. We thus obtain a clear idea of this definite point in the sky, and the horizon is a perfectly definite circle, at all events from any station where the sea is visible. It is not difficult to imagine that by suitable measurements we can ascertain the altitude of this point in the heavens above the horizon. That altitude is the latitude of the place; it is, in fact, the very angle which lies between the locality on the earth and the Equator.

It is quite true that as the Pole is implied by these circles rather than directly marked by them, the measurement of the altitude cannot be effected quite directly. The actual process is to take the Polar Star, or some one of the other circumpolar stars, and to measure the greatest height to which it ascends above the horizon and the lowest altitude to which it declines about twelve hours later. The former of these is as much above the Pole as the latter is below it, so between them we are able to ascertain the altitude of the Pole with a high degree of accuracy.

It is true that in a fixed observatory such as Greenwich there is no visible sea horizon, and even if there were it would not provide so excellent a method as is offered by the equivalent process of first observing the star directly and then observing its reflection in a dish of mercury. In this way the altitude of the star above the horizon is determined with the utmost precision. The practical astronomer will, however, remember that, of course, he has to attend to the effects of atmospheric refraction, which invariably shows a star higher up than it ought to be. This can be allowed for, and in this way the latitude of the observatory is ascertained with all needful accuracy. When the highest degree of precision is sought for, and it is only observations with a very high degree of precision which are available for our present purpose, a considerable number of stars have to be employed, and very many observations have to be taken at different seasons of the year, so as to eliminate as far as possible all sources of casual error. When, however, due attention has been paid to those precautions which the experience of astronomers suggests, the result that is obtained is characterized by extraordinary precision.

How great that precision may be I must endeavour to explain. The latitude of every important observatory is obtained from a large number of observations, and it would be unlikely that it was more than one or two-tenths of a second different from the actual mean value. Now a tenth of a second on the surface of the earth corresponds to a distance of about ten feet, and this means that the latitude of the observatory or, as we must now speak very precisely, the latitude of the centre of the meridian circle in the observatory, is known to a degree of precision represented by a few paces. It will thus be seen that, with the accuracy attainable in our modern observation, it would often be an appreciable blunder to mistake the latitude of one wall of the observatory for that of the opposite wall; in other words, we know accurately to within the tenth of a second, or within not much more than the tenth of a second, the distance from the centre of the transit circle at Greenwich down to the earth's Equator. But, of course, the distance from the Pole to the Equator is 90°, and this being so it follows that the distance from the North Pole of the earth to the centre of the transit circle at Greenwich Observatory has been accurately ascertained within one or two tenths of a second. If any change took place in the distance between the Pole and the meridian circle at Greenwich, then it must be manifested by the changes of latitude. We shall now be able to understand how any movement of the Pole, or rather of the position which it occupies in the earth, would be indicated at Greenwich.

Suppose, for instance, that the Pole actually advanced towards Great Britain, and that it moved a distance of, let us say, thirty feet, the effect of this would be to

produce a diminution of the distance between the Pole and Greenwich, that is to say, there must be an increase in the distance from Greenwich to the Equator. This corresponds to a change in the latitude of Greenwich; in fact it would diminish by three-tenths of a second, which is a magnitude quite large enough to be recognisable by the observations I have already indicated as proper for the determination of latitude. A shift of the Pole to a distance of sixty feet would be a conspicuous alteration announced in every observatory in Europe provided with instruments of good modern construction.

Until the last few years there was not much reason to think that the Pole exhibited any unequivocal indications of movement. No doubt, displacements resembling those which have now been definitely ascertained have existed for many years, but they were too small to produce any appreciable effect, except on instruments of a more refined description than those with which the earlier observatories were equipped. It was obvious that the Pole did not make movements of anything like a hundred yards in extent; had it done so the resulting variations in latitude would have been conspicuous enough to have obtained notice many years ago. The actual movements which the Pole does make are of that small character which require very minute discussion of the observations to establish them beyond reach of cavil. There is, however, one striking method of confirming such observations as have been made which leaves no doubt of the accuracy of the results to which they point.

If the observatories in Europe indicated at a certain time that their latitudes had all increased; this would imply that the Equator had receded from them, and

that, therefore, the North Pole had approached Europe If, however, the North Pole had approached Europe it must have retreated from those regions on the opposite side of the world—say, for instance, the Sandwich Islands. Observations in the Sandwich Islands should, therefore, indicate, if our reasoning has been correct, that the Pole had retreated from them, and that the Equator had, therefore, advanced in such a way that the latitudes of localities in the Sandwich Islands had diminished. The various observations which have been brought together by the diligence of Mr. Chandler, including those which he has himself made with an ingenious apparatus of his own design, have been submitted to this test, and they have borne it well. The result has been that it is now possible to follow the movements of the Pole with a considerable degree of completeness. Professor Chandler has tracked the Pole month after month, year after year, through a period of more than a century of exact observations, and he has succeeded in determining the movements which this point undergoes. Let me here endeavour to describe the result at which he has arrived.

Situated in that palæocrystic region which Arctic travellers have so long essayed to enter, but hitherto without success, is that interesting North Pole which is the object of so much speculation. With a particular centre in this region let a circle be supposed to be drawn the radius of which is about thirty feet. In the circumference of this circle the Pole of the earth is constantly to be found. In fact, if at different epochs, month after month and year after year, the position of the Pole was ascertained as the extremity of that tube from

which an eye placed at the centre of the earth would be able to see the Pole of the heavens, and if the successive positions of this Pole were marked by pegs driven into the ground, then the several positions in which the Pole would be found must necessarily trace out the circumference of the circle that has been thus described. The period in which each revolution of the Pole around the circle takes place is about 427 days; the result, therefore, of these investigations is to show that the North Pole of the earth is not, as has been so long supposed, a fixed point, but that it revolves around in the earth, accomplishing each revolution in about two months more than the period that the earth requires for the performance of each revolution around the sun.

The detection of the movement of the Pole which I have here described must be regarded as a noteworthy achievement in astronomy, nor is the result to which it leads solely of interest in consequence of the lesson it teaches with regard to the circumstances of the earth's rotation. It has a higher utility, which the practical astronomer will not be slow to appreciate, and of which he has, indeed, already experienced the benefit. There are several astronomical investigations in which the latitude of the observatory enters as a significant element. Latitude is, in fact, at every moment employed as an important factor in many astronomical determinations: to take one of the most simple cases, suppose that we are finding the place of a planet, we deduce its position by measuring its zenith distance, and then to obtain the declination the latitude of the observatory has, of course, to be considered. Now, astronomers have hitherto been in the habit of accepting the determination of their latitude

which has been established by a protracted series of observations, and treating it as if it were a constant. This method will be no longer admissible in astronomical work of the highest class. No doubt, from the sailor's point of view, an alteration in latitude which at most amounts to a shift of sixty feet, not a quarter, perhaps, of the length of his vessel, is immaterial. But in the more refined parts of astronomical work these discoveries can no longer be overlooked; indeed, Mr. Chandler has shown that many discrepancies by which astronomers have been baffled, can be removed when note is taken of the circumstance that the latitude of the observatory is in incessant alteration in accordance with the law which his labours have expounded. It will ere long be necessary, in every observatory where important work is being done, to apply each day the correction to the mean value of the latitude, in order to obtain the value appropriate for that day.

There are also other grounds of a somewhat profounder character on which the discoveries now made are eminently instructive. Those who are interested in the physics of our globe often discuss the question as to whether the internal heat, which the earth certainly possesses, is sufficiently intense to render the deep-seated portions of our globe more or less fluid. On the other hand, the effects of pressure, especially of such pressures as are experienced in the depths hundreds and thousands of miles below the surface, must go far to consolidate the materials to form what must resemble a rigid body. The question, therefore, arises, Is the earth to be regarded as a rigid mass, or is it not? The phenomena of the tides had already to some extent afforded information on this subject, and now Mr. Chandler's investigation adds much

further light, for it is certain from his result that the earth cannot be a rigid body. It is quite true that, even though the earth were rigid, the Pole might revolve in a circle, and that circle might have a thirty-feet radius, but in such a case the period would be only about three-quarters of the 427 days which he has found. In the interest, therefore, of the theoretical astronomer, as well as on the other grounds which I have set forth, Mr. Chandler's investigations must be regarded as a most important contribution to modern astronomy.

CHAPTER IV.

THE GREAT ECLIPSE OF 1893.

THE total eclipse of the sun which took place on April 15th—16th, 1893, is in some respects the most remarkable event of the kind in the present century; certainly no other like phenomenon occurring within the next decade will equal it in the presentation of exceptionally favourable conditions.

It is obvious that there are two criteria by which we may judge of the suitability of an eclipse of the sun for the purposes of the astronomer; the first relates to the astronomical conditions, and the second to those of a merely geographical character. Of course it must be understood that any eclipse which would disclose information sufficient to justify despatching an expedition for thousands of miles must be total. There is but little to be learned from any observations at a place from which the disc of the sun appears only partly obscured by the interposition of the moon. Such an opportunity may, indeed, enable accurate determinations of the relative positions of the sun and the moon to be obtained, and these

are often of service in our efforts to improve the tables by which the movements of the moon are calculated. But this object is of very slight importance compared with those which chiefly occupy our attention during a total eclipse. The primary question in determining the astronomical value of a total eclipse relates to the duration of the phase in which the obscurity is total.

Tested by this standard, the phenomenon which we are now to consider is one of exceptional value. The phase of "totality" lasted for 4 minutes 40 seconds on the east coast of Brazil. This may seem, indeed, but a short time in which to commence and complete an elaborate series of observations and measurements; but by skilful organization of the work it is now possible for a corps of experienced observers to effect, even in this very limited time, an amount of careful work that would greatly surprise anyone who was not acquainted with the resources of modern scientific methods. Indeed, on former occasions many successful eclipse observations have been made when the period of totality has been much less than that just stated. Even in the event which we are now considering, other stations at which the duration of totality was much below the maximum have been occupied with much advantage. Thus in Chili totality lasted for 2 minutes 56 seconds. It was 9 seconds longer in Argentina. It reached the maximum for available terrestrial statistics on the east coast of Brazil; but the actual maximum duration of 4 minutes 48 seconds would be observed from a point some hundreds of miles off in the Atlantic. On the west coast of Africa, at Senegal, the duration was 4 minutes 10 seconds.

To realise the conditions under which an eclipse is

produced we must remark that, wherever the moon may happen to be, it bears at all times a long conical shadow projected behind it, sometimes into space, sometimes towards our globe. The cone tapers to a point at a distance which varies somewhat, but is about a quarter of a million miles from the moon. For the production of a total eclipse of the sun it is necessary that the eye which observes should be somewhere within the cone of shadow. Even when the moon does come in between the earth and the sun it will sometimes happen that the shadow-cone is too short to touch the earth, in which case an annular eclipse will result. Sometimes, however, owing to the varying distances of the sun and the moon from the earth this cone does extend far enough to reach the earth, and then observers who happen to occupy any spot in the shadow will have a total eclipse presented to them.

About 1 P.M., Greenwich time, on Sunday, 16th April, the sun was rising in the Pacific Ocean in a state of total eclipse, the moon casting a deep black shadow on the shining waters around. There does not happen to be any island lying near enough to the critical position for its inhabitants to have witnessed this interesting phase of the spectacle: Juan Fernandez was too far to the south and St. Ambrose too far to the north. The region of complete obscuration was at first oval in form, and the shortest diameter extended some ninety miles north and south. The black patch then commenced its great eastward journey, and presently reached land on the coast of South America. The local time was then about half-past seven in the morning at the point of arrival on the coast of Chili, in 30° south latitude. Professor Pickering was among the first of an ardent corps of astronomers ready

to greet the total eclipse and to utilise to the utmost the advantages of an early station. Then the shadow began its journey across the South American Continent.

With a speed of something like 3,000 miles an hour, far swifter than any rifle bullet ever moved, the silent obscurity sweeps across wide deserts in the interior, and then over the noble rivers and glorious forests of Brazil, to quit the land after barely an hour has been occupied in the transit. Along its track it has been watched in two or three places by interested observers armed with spectroscopes, photographic cameras, and the other paraphernalia of the modern astronomer. Doubtless the sudden gloom caused no little dismay to many a tribe of savages in the deep interior of tropical America. We may also conjecture that other creatures besides man had their share of astonishment. Darwin and Bates have charmed all readers by their exquisite delineation of these virgin forests of Brazil, where organic nature is developed with a luxuriance which those whose rambles have been confined to sterner climes can hardly realise. Probably in Brazil, as elsewhere under similar conditions, tender plants evinced their belief that night had prematurely arrived. Beautiful flowers no doubt closed their petals as they are wont to do after sunset. Other flowers, again, which open out at night to solicit the attention of moths, to whom the darkness is congenial, doubtless began to expand their charms. With the advancing gloom such plants as emit their delicious perfume only when the glory of the day has vanished were likewise deceived, as they have been known to be on other occasions of a like kind.

We can also speculate on the amazement which the total eclipse must have produced among the various races

Fig. 11.—Path of the Moon's Shadow and Penumbra upon the surface of the Earth during the total Eclipse of the Sun, April 15th—16th, 1893.

of animals. The great flocks of Brazilian macaws must have wondered why the time for going to roost had indeed arrived again so soon. The chattering monkeys and the skulking jaguar would have been sorely puzzled; while the marvellous nocturnal insect life which Mr. Bates has so forcibly described would have been deceived into temporary vitality. For some minutes, it may be reasonably assumed that the forest depths resounded with those myriad notes of crickets and grasshoppers which appear to be one of the most striking features of night in the tropics.

Quitting the east coast of America, the lunar shadow took an Atlantic voyage. It crossed the ocean at perhaps its narrowest part, and may have buried in its gloom

many a vessel, whose crew gazed with astonishment at the unwonted spectacle. Here the conditions of good observation, so far as celestial requirements are concerned, would have been of the most desirable nature. The sun would have been overhead and the fervid glories of the equatorial noon would have been suspended for the space of nearly five minutes. Splendid indeed must have been the view of the corona obtained by those who were fortunate enough to be in the right position on the ocean, with a clear sky overhead. But from the astronomer's point of view the observations which can be made on board ship are of but little importance; the deck does not offer the stable foundations that are required for elaborate photographic or spectroscopic apparatus.

For the space of an hour, therefore, while this ocean passage was in progress, there were but few opportunities, if indeed any, for valuable contributions of facts to illustrate our theories of the corona. The speed with which the shadow traversed the sea happened to be not so great as that with which it crossed South America. The consequence was, that rather more than an hour was occupied by its journey from the American to the African coast. This ocean distance is only about half as long as the track pursued across the South American continent; but owing to the curvature of the earth the incidence of the shadow makes it travel more quickly at the beginning or the end than in the intermediate stages, so that in consequence of the decline in speed about the middle of the eclipse, the time required by the ocean journey was about the same as that needed for the previous land journey.

A few minutes after half-past three, Greenwich time,

on Sunday, 16th, the shadow reached land again on the African coast near the River Gambia, about north latitude 15°. Here the eclipse was destined to receive a cordial welcome from the bands of astronomers who were ready to receive it. Sweeping onwards with a pace which had now begun again to accelerate, the shadow advanced into the interior of Africa, keeping below the parallel of 20°, and gradually curving southwards. At four o'clock on Sunday afternoon, the position from which totality was to be observed had gone to the east of the meridian of Greenwich. The end of the phenomenon was now rapidly approaching; the last glimpse that could be had of it from this earth would have been from the desert of Sahara, where, just at the moment of sunset, the phase of totality was reached. At a quarter past four, Greenwich time, the eclipse ceased to be total anywhere, but an hour longer had yet to elapse before the partial eclipse had vanished from the globe. It will, of course, be understood that at any particular locality the total eclipse only lasted for the time that the shadow occupied in passing over that locality. Thus the duration at any particular station was only about as many minutes as the hours during which the total eclipse required for its terrestrial journey.

It is plain that the best sites, so far as astronomical conditions are concerned, must be those where the duration of totality is as long as circumstances permit. To secure this, on the occasion now before us we had to occupy sites which lay as nearly as possible along the middle of a strip eighty miles wide, extending from the South Pacific to the middle of the Sahara. It fortunately happens that on this occasion those localities where the

astronomical conditions were favourable also turned out to be those where the geographical conditions were suitable and comparatively convenient. At Chili, in Argentina, in Brazil, and on the African coast, astronomers were able to obtain a series of admirable pictures, not often paralleled in eclipse observations. One special advantage offered by this chain of observing stations should be particularly noticed. It is a question of considerable importance to examine the nature of the changes which take place in the corona. It has sometimes been thought that such changes frequently occur with extreme rapidity. No doubt, when we remember the scale of the objects involved, it will hardly be imagined that in the brief interval of four or five minutes, during which the eclipse lasted, any variation in the corona should have taken place considerable enough to be recognised from the distance at which we are placed. We thus have the opportunity of carefully examining whatever changes may have taken place in the corona in the interval between the time of totality in Chili and the time of totality in Africa. As we have pointed out, this period is no less than two and a half hours. In this respect, the advantage offered by the eclipse of 1893 is almost unique, for though on other occasions observations of totality may have been possible for a number of seconds greater than those at either of the stations we have named, yet the circumstance of having in the same eclipse two occupied stations so widely separated as the western coast of North Africa and the western coast of South America is quite an exceptional advantage.

And now as to the problems which astronomers have proposed to themselves to solve when undertaking the

observations of this as indeed of certain other recent eclipses of the sun.

The history of modern astronomy makes it plain that a remarkable change has taken place in the nature of the questions which specially demand attention during such

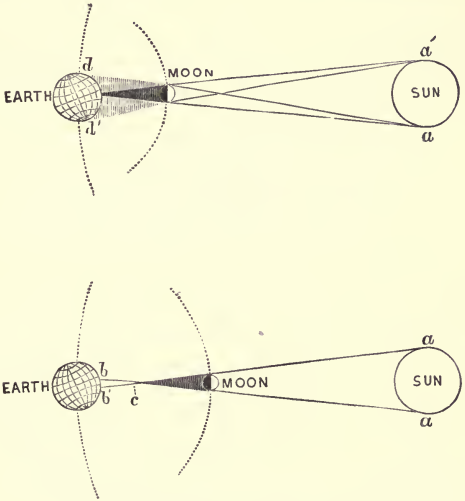

Fig. 12.—Total and Annular Eclipses.

phenomena. Twenty-four years ago a total eclipse was regarded as of special value, as it afforded the opportunity of investigating those remarkable prominences or coloured flames round the sun's margin which were then considered to be not otherwise visible than by the occurrence of a total eclipse. Attention was no doubt also directed in the earlier eclipses to the

silvery corona which stretched from the sun to such a vast distance into the surrounding space. The corona, though a permanent appendage of the sun, was only to be recognised when by the direct interposition of the moon the light of the sun was cut off, and in the gloom thus arising the radiance of the corona became readily and even brightly discernible. But the memorable discovery made by Janssen and Lockyer, independently, in 1868, showed that the prominences could be observed without the help of an eclipse, by the happy employment of the peculiar refrangibility of the rosy light which these prominences emit. This improvement in observational astronomy revolutionised the method of utilising eclipses. We are now so well acquainted with the forms of the prominences by the spectroscopic method that the eclipses have but little to teach us on that matter.

Of course it will be admitted that there are many circumstances with regard to these objects as to which we at present know but very little; however, we do not look in any considerable degree to eclipses for their solution. Quite recently a further extension has been given to the spectroscopic method of studying solar prominences by the beautiful invention of Professor Hale of Chicago. He has employed a very elaborate apparatus by which he is able, as it were, to sift out from the sunlight the beams of that particular refrangibility which astronomers would denote by saying that it belonged to the H line of the spectrum. With the light thus chosen Professor Hale obtains a photograph. It so happens that in the light of this particular hue—an invisible hue, it may be added, only perceptible to the peculiar sensibility of the photographic plate—the prominences are peculiarly rich. It

follows that when all other light is withdrawn, as Professor Hale's method enables him to withdraw it, the ordinary solar light remaining has become so much weakened that it can no longer overpower the beams from the prominences, and hence an image is printed on the photographic plate. Thus we can now obtain, not as heretofore merely isolated views of special prominences through the widely opened slit of the spectroscope, but we are furnished, after a couple of minutes' exposure, with a complete photograph of the prominences surrounding the sun. In Professor Hale's remarkable pictures, not only is every large prominence exhibited with ample detail, but the incandescent region of the chromosphere from which these prominences arise is also recorded with accuracy.

It may therefore be said that with this admirable process available the eclipse is no longer of much account for the purpose of instructing us as to the prominences. No doubt a pleasing picture of these objects may be afforded. Professor Pickering, indeed, describes them as of much interest on the recent occasion; but the attention of the eclipse observer in the present day is almost wholly otherwise directed.

For the corona is still only known to us by such opportunities as eclipses present. No doubt attempts have been made by photographic methods of various kinds to enable the corona to be brought within our scrutiny under ordinary circumstances. Up to the present, however, success cannot be said to have rewarded these efforts. The sunlight is so intense that if it be reduced sufficiently by any artifice, the coronal light also suffers so much abatement that, owing to its initial feebleness, it ceases altogether to be visible. We are therefore wholly depen-

dent on eclipses for accessions to our knowledge of the corona, so it will not be a matter of surprise that on the recent occasion the attention of the different parties was almost entirely concentrated on the minute scrutiny of the corona by every device which was likely to explain its nature.

The astronomers of Great Britain had as usual taken a leading part in organizing plans for the purpose of observing this eclipse. A joint committee of the Royal Society, and of the Royal Astronomical Society, had general charge of the arrangements. The sinews of war were chiefly provided from that liberal grant of £4,000 a year which the State places at the disposal of the Royal Society for furthering the interests of science in such ways as may seem most advantageous.

Assistance of other kinds was also forthcoming. In many cases the owners of valuable instruments placed them at the disposal of the observers. The Admiralty provided such facilities of transport as were needed to attain out-of-the-way places. The comity of nations was also illustrated by the readiness with which the authorities of the French and Brazilian Governments respectively complied with the requests made to them. They afforded accommodation and extended courtesies to the parties on the coast of Brazil and in the French territory on the African coast to which the two British expeditions were despatched.

A careful study of the meteorological conditions of the different localities was a necessary preliminary to the choice of stations. For it need hardly be said that, however suitable a station may have appeared to be from the astronomical facts of long duration and of high altitude

of sun, yet if the locality in question were one likely to be obscured by clouds it would be somewhat improvident to despatch an expedition to a place where the chances of success would be so greatly jeopardised.

Perhaps the most elaborate study of the meteorological conditions bearing on the question was contained in a paper contributed by Prof. David P. Todd to the *Meteorological Journal.* In this he has brought together a mass of information collected from divers authentic sources. The inhabitants of Chili were able to report that the observations of any celestial phenomena which take place in April are almost certain to be made in a cloudless sky. Indeed it appears that in the mountainous regions of that favoured climate the atmospheric conditions are almost ideally perfect for the purposes of the astronomer. The course of the shadow then lay through Argentina, where the residents assure us that April is the best month of the year for clear atmospheres and light skies, and that it could only be through some exceptional misfortune that the observers would meet with disappointment. In Paraguay, which the shadow next traversed, it seems that meteorological zeal has not yet been kindled. No accurate information as to the clouds or weather to be expected in April was forthcoming in response to Mr. Todd's urgent inquiries. In despair of being able to offer climatic inducements to the expedition he expressed a hope that any eclipse party despatched thither might include zealous naturalists. The attractions offered by pumas, jaguars, cobras and cross vipers in abundance might in that case suffice to "offset the possible loss of the corona to the astronomer." But it may well be doubted whether the enthusiasm of the astronomer, who studies with much

interest Serpens and Draco in the skies, would be sufficient to induce him to journey all the way to Paraguay in expectation of becoming acquainted with their terrestrial representatives, possibly on closer terms than he could desire.

In Brazil, where the astronomical conditions are of the best, the risk of clouds was considerable. It seems that about half of the days in April on the coast at Para Cura are likely to be obscured. Fortunately, however, the observers were favoured with good weather. Pains had also been taken to determine the chances of cloudiness at this season along the Atlantic track followed by the shadow. They showed that the probability of a clear sky at mid-day in April at any point along the track followed by the eclipse from Ceara, where the central line leaves the coast in Brazil, to Gambia, on the other side of the Atlantic, is about one half.

It was not possible to obtain any very definite information as to the extent of April cloudiness in the interior parts of Africa which were passed over by the lunar shadow ere it finally quitted the earth. It seems, however, impossible to doubt that an expedition might have been despatched to some locality in the far interior of Senegal or the Sahara, where the atmospheric conditions would have been excellent. A continuous chain of observations of the corona would then have been available from the time the sun was rising on the coast of South America to the time of sunset in Sahara. The great advantage of such an expedition would have been that it would have afforded an opportunity for testing in the completest manner whether the corona submitted to these rapid changes in the few hours to which we have already

referred. The eclipse now under consideration was admirably suited for this investigation, for the terrestrial conditions were such as to enable the observations to be made both near the beginning and the end of the phenomenon. Further, as the sun spots were at the time very abundant, it might be presumed that the sun was in a condition of exceptional activity, and consequently it seemed reasonable to suppose that, in sympathy with what was going on below, the corona would be in a disturbed state. Unfortunately, however, it was not found practicable to make use of the extreme end of the track of the shadow.

The English Brazilian party, consisting of Messrs. Taylor and Shackleton, were stationed at Para Cura. The African party was organized on a somewhat larger scale. Professor Thorpe was placed in command of it, and he was accompanied by Lieutenant Hills, R.E., Sergeant Kearney, R.E., and Messrs. Fowler, Gray, and Forbes, from the Royal College of Science. They were despatched to Bathurst, thence to make their way to a station in French Senegambia only a few miles south of the central line of totality.

As the pictures of the corona vary so much with the instrument employed, it is clearly desirable to have some means of discriminating between the actual changes which may have taken place in the structure of the corona itself between one eclipse and the next, and those differences in the representation of it which merely arise from instrumental causes. There is no means of attaining this end so simple and so secure as to provide that the same photographic apparatus shall be used on each occasion. For this reason the corona was photographed in

Africa on Sunday, the 16th instant, with the same 4-inch lens of 60 inches focus which was used in Egypt in 1882, in the Caroline Islands in 1883, in Granada in 1886, and in the Salut Islands in 1889. We have thus a connected series of pictures of the corona, taken as far as possible under similar conditions, extending over a period of eleven years.

Particular interest will be attached to the department of work assigned to Mr. Fowler in Africa. He photographed the spectrum of the corona, produced by placing a glass prism in front of an object-glass of six inches aperture. The peculiar advantage of this method of observing is that for each source of light of special refrangibility in the corona a distinct image of the corona will be impressed on the plate. If, for example, the coronal light was of that strictly monochromatic type of which the light of certain nebulæ appears to be, then the coronal photograph as produced through the prism would represent the details of the structure in a single definite picture. If, however, as seems much more likely, the corona diffused light of two or more refrangibilities, then separate pictures of it would be obtained in distinct positions on the plate, in correspondence with each of the constituent rays. The several pictures thus obtained would be indications of the different kinds of light of which the corona was composed. So far as these various simulacra can be discriminated and interpreted they will afford indications of the material constituents of the luminous substances from which they originate. It need not be expected that these several pictures will resemble each other. If the different parts of the corona contain different elements in their constitution, as is cer-

tainly most probable, then the several pictures will evidence this by their difference in outline. No doubt the different photographs may to some extent overlap, but though this will interfere with the pictorial effect, it will not prevent their interpretation in the sense that it is instructive to the astronomer.

One of the most remarkable features in the structure of the corona is the presence of streamers or luminous rays extending from the north and south poles of the sun. These rays are generally more or less curved, and it is doubtful whether the phenomena they exhibit are not in some way a consequence of the rotation of the sun. This consideration is connected with the question as to how far the corona itself shares in that rotation of the sun with which astronomers are familiar. I should perhaps rather have said—that rotation of the sun's photosphere, which, as the sun-spots prove, is accomplished once every twenty-five days. Even this shell of luminous matter does not revolve as a rigid mass would do. By some mysterious law the equatorial portions accomplish their revolution in a shorter period than is required by those zones of the photosphere which lie nearer the north and south poles of the luminary. As to how the parts of the sun which are interior to the photosphere may revolve, we are quite ignorant. Nor does there seem much likelihood of any discoveries being made which will clear up this matter. We have no means of knowing to what extent the corona shares in the rotation. It would seem certain that the lower parts which lie comparatively near the surface must be affected by the rapid rotation of the photosphere. But it is very far from certain that this rotation can be shared in to any great extent by those parts of the corona

which lie at a distance from the sun's surface as great as the solar radius or diameter.

The spectroscopic testimony forms of course an exclusive source of information as to the nature of the elementary bodies present in the corona. It must be admitted that our knowledge on this subject is rather of a negative character. The spectroscope has hitherto mainly afforded us indications of elements which seem to be undeterminable by our knowledge of terrestrial chemistry. Professor Schuster, after a careful discussion of the evidence afforded by other eclipses, has come to the conclusion that it is not at present possible to identify the lines specially characteristic of the coronal spectrum with those of any known terrestrial substances. Indeed, the corona presents a curious green line that seems to denote some invariable constituent in the sun's outer atmosphere; but the element to which this green line owes its origin is wholly unknown. It has been conjectured that it is due to some body present in the sun which is unknown to terrestrial chemists. The elucidation of this question is from every point of view one of the most interesting problems in solar physics.

CHAPTER V.

THE FIFTH SATELLITE OF JUPITER.

SINCE the invention of the telescope some two hundred and eighty years ago the great planet Jupiter has never been the object of so much attention as it was during the autumn of 1892. It will be remembered that among the first-fruits of the new instrument for looking at objects which were a long way off, was the great discovery of the system of which Jupiter was the centre. The four satellites lie just on the dividing line between objects which can be seen with the unaided eye, and objects which require optical assistance to make them visible. It seems to be certain that there have been individuals gifted with rare powers of vision who under exceptionally favourable circumstances have been able to discern one or other of the satellites of Jupiter without optical aid. Testimony has been adduced which seems to show that long before the invention of Galileo's tube for studying the heavens, one or two of these satellites had been seen by the Chinese. But it would be futile to say that these glimpses of the moons of Jupiter really amounted to any anticipation of the great discovery of Galileo. Such mere casual observations never thoroughly

demonstrated the existence of the little bodies, still less did they yield such a volume of accurate knowledge as would enable us to determine their movements, so as to say when they would again be likely to be visible. No astronomer ever seriously entertained the notion that there was such a system of attendants revolving around Jupiter until their existence had been demonstrated once and for all by the telescope of Galileo.

There can be no doubt that the moons of Jupiter are in themselves quite bright enough to be ordinarily seen by the unaided eye were it not for a single circumstance. They lie too close to the great planet. At first sight it might seem that the very fact that they are placed in the brilliant illumination which Jupiter radiates should rather tend to make them more easily discerned. The nearer an object is held to a source of light the better it can generally be seen. Does it not therefore appear somewhat paradoxical to say that the reason we are generally unable to see the moons of Jupiter with the unaided eye, is because they lie so close to a lustrous globe? Ought not that to be the very reason why they should be seen with all the greater facility? This is a point which may require a few words of explanation because it is intimately connected with the recent great discovery.

We hold a book near a candle when we want to read, because under the circumstances supposed the only light available comes from the candle. The type has no other illumination, and the nearer it is to the candle the clearer the printing appears. But this is not at all analogous to the case of Jupiter and his satellites. We cannot think of Jupiter as the candle, and a satellite as a page of the book. If such were indeed the analogy, then the nearer

the satellite lay to the body of the planet the more brightly would it be illuminated, and under certain circumstances the more easily would it be seen. The very opposite is, however, notoriously the case. It will be instructive to see wherein the difference lies.

It is, of course, quite true that Jupiter is a brilliant source of light, but in this respect the light that emanates from the planet is of a very different character from the light which radiates from the sun. Jupiter is not a sun-like object diffusing radiance around in virtue of his own intrinsic brilliancy. If the satellites revolving around the planet were really in the condition of the planets themselves as they revolve around the sun, then the closer the satellite was to the planet the greater would be the illumination it would receive. But of course this is not at all the case. Although we have excellent reasons for believing that Jupiter is in truth a highly heated globe, yet we are perfectly assured that the temperature falls far short of that which would be required if the great planet were to dispense its beams around in the same manner as a miniature sun. Indeed, there are facts connected with the satellites themselves which render it perfectly clear that Jupiter, so far from possessing a sun-like radiance, is absolutely devoid of any intrinsic light whatever. It shines merely as the earth itself shines, or as the moon shines, or the planet Mars, or Venus, or Saturn, by the sunbeams which fall upon it. As to the light which illumines the satellites of Jupiter, it also can only come from the sun. We cannot, indeed, say that the light-radiating power of a certain area on Jupiter is the same as that from an equal area on one of his moons. There are certainly intrinsic differences between the material

constitution of the great planet and of his satellites which prevent us from affirming that they are at all times equally capable of reflecting light. Some portions are whiter than others, and therefore return a larger fraction of the sunlight which falls upon them. Still, however, we may for the purpose of the present argument remember that as both Jupiter and the moons are illuminated by the same sun, they are both sufficiently nearly of the same brightness. It therefore follows that there would be no gain of lustre to the satellite in being near Jupiter. Note, then, the difference between what would have happened if Jupiter were sun-like, and what does actually happen when Jupiter is merely a planet. In the former case there would be a distinct accession of brightness to the satellite the closer it made its approach. In the latter case there would be no variation of brightness at all.

To follow the argument a step further we must think of what takes place in the eye of the astronomer who is observing the planet. On the retina an image is formed ot the great globe. The image is extremely minute, but owing chiefly to the disturbances of our atmosphere, the genuine image is surrounded with a region in which the nerves of the retina are more or less affected by the light, which ought, if all the sources of disturbance could be excluded, to be entirely concentrated within the image itself. Of course, vision of other objects on this affected part of the retina will be correspondingly impaired. If the image of a satellite fall upon it, then whether it will be perceived or not depends upon whether the brilliance of the little object is sufficient to excite those nerves whose sensibility is somewhat lessened by the stray light referred to As the satellite acquires no increased bril-

liance from Jupiter's own lustre, it is obvious that the further it be away the more will its image stand out on that part of the retina where there is no temporary diminution of sensibility, and the better it will be seen. Such is the reason why the moons of Jupiter cannot be seen with the unaided eye except under conditions that need not be again referred to. Were objects of no greater brilliance quite aloof from such a bright orb as one of the great planets, they would easily be discerned without optical aid. In the ordinary language of the astronomer, one of Jupiter's satellites would be reckoned as bright as a star of the fourth magnitude.

Since the discovery of the satellites of Mars by Professor Asaph Hall at Washington, in 1877, there has been no event in the astronomical world which has possessed the same interest as that of which we are now speaking. From one point of view it might appear that the announcement of an addition of another satellite to Jupiter's system was no very significant matter after all. No doubt the new attendant of the great planet is a very trifling object, as far as dimensions are concerned. It is not nearly so large as many of those minor planets the discoveries of which are constantly being announced. How comes it that people are talking about and thinking about Jupiter's fifth satellite, while there are thousands or rather millions of stars lying unnoticed through space? and yet any one of these stars is perhaps a million times as big as this little satellite, besides being a sun, which may presumably be a source of light and heat to planets which circulate around it. What is the ground for so much excitement about the discovery of an object which is probably among the most minute, if not itself actually the most

minute, of all the objects of which our telescopes can take cognisance? Why is it that an apparition of a great comet with a blazing tail half across the sky would not have for astronomers half the interest possessed by this little stranger? Why is it that sun-spots, lunar craters, the ice-caps of Mars, and his newly discovered lakes, the revelations as to Venus, were for the moment forgotten or unheeded; why did everyone become so eager to learn all about this tiny little moon of Jupiter, and on the tiptoe of expectation for every further item of intelligence from the Lick Observatory?

Let me say at once that this extreme interest in the little object appears most natural. I am glad to share in it, for there seem to be certain very good reasons why such interest should be felt. In the first place, Jupiter has always been a favourite telescopic object. The globe itself is so vast that the features are on a sufficiently large scale to be discernible with comparatively small instrumental power. Thus it is that the cloud belts on the great planet are familiar objects to everyone who has ever used a telescope. Then, too, the ever-varying positions of the four older satellites make them a spectacle that is always attractive. At one time we are watching an eclipse, in which a satellite plunges into Jupiter's shadow and disappears for a while. At another time we note how the wanderer vanishes by occultation behind the body of the great globe around which it revolves. Then, too, there are the singularly delicate and beautiful phenomena of the transits of the satellites in front of the planet. In this case not only is the satellite itself often to be traced in the act of crossing, but, as a far more striking manifestation, its dark shadow is thrown on the brilliant globe. The

movements of the satellites are so rapid that the different phenomena we have referred to are repeated frequently, and many of them can be discerned with a comparatively small telescope.

But the Jovian system of satellites has also a claim on those astronomers who devote themselves rather to mathematical research than to telescopic observation. Each of the moons is, of course, mainly guided in its movement by the attraction of the great globe itself. If there were only a single moon and if there were no other interference, then the determination of its movement would be a comparatively simple matter, and the places occupied by the satellite at every date could be predicted with complete confidence. In no one of the planets, however, can so simple a condition of things as we have supposed be realised. It is no doubt true that the earth is attended by but a single moon, but then the movement of the earth's companion is rendered highly complex by the fact that the attraction of the sun constantly tends to make it swerve from the simple elliptic path which it would otherwise pursue. The movement of our moon, however embarrassing a problem it may present to mathematicians, is, nevertheless, simplicity itself in comparison with the movement which has to be performed by one of Jupiter's moons. In the case of each of these little bodies there is not only a force exerted by the sun with the effect of disturbing the satellite's motion, but each one of the four globes attracts each one of the others, and is in turn attracted by it.

The consequence of this is that the movements of Jupiter's satellites form one of the most troublesome problems which the mathematical astronomer has ever

been called upon to solve. It presents peculiar features of difficulty arising from the exceptional character of the Jovian system; but these very difficulties, so far from deterring mathematicians from the study, have in some cases acted as a stimulus. A considerable part of Laplace's famous book, the "Mécanique Céleste," is devoted to the study of the system of Jupiter's satellites. He has contrived certain analytical methods for encountering the many points that arise, and he has succeeded in explaining some of the most remarkable dynamical features of the system. Many other mathematicians have also essayed the task of a thorough elucidation of the problem. Indeed, on more than one occasion the question has been propounded by the Academy of Sciences at Paris, and a considerable prize has been offered for a satisfactory discussion.

The test of the completeness of such a theory would be sought in the precision with which it would enable the movements of the satellites to be predicted. No doubt a good deal has been done in this way. Our "Nautical Almanac," for instance, announces, with all needful details, the various eclipses and occultations of the satellites, as well as their transits and the movements of the shadows across the disc. It is quite possible, even with our present knowledge of the subject, to predict such phenomena for some years in advance. The accuracy with which these indications can be made is amply sufficient for the ordinary purposes for which they are required. But in such investigations the requirements of science demand a much closer degree of approximation between what is observed and what is calculated than is possible in the present state of our

knowledge of Jupiter's satellites. There is much work yet to be done before the movements of this system can be reduced to satisfactory order.

The object in now mentioning these matters is not assuredly to attempt, in these pages, any contribution towards the task of improving the tables of Jupiter's satellites. My purpose is to show how much attention has been paid to the system by astronomers of every class. It would be utterly impossible to obtain any accurate notion of how often Jupiter has been carefully observed, let us say, within the last hundred years. We can, however, obtain some sort of estimate which will help to explain the profound impression that the announcement of a discovery of a new satellite is calculated to make. The great planet is visible for some months every year. We shall certainly be well within the mark if we say that it must have been scrutinised carefully by skilful observers at least a thousand times each year. For, remember how many observatories there are in the world, where special attention is given to such work, and also of late years how many excellent telescopes there are in private hands. Considering, too, that the Jovian system is one of such intense interest to all observers, and that, except the moon, there is no object in the sky more frequently and more carefully studied, it is not at all an undue estimate to assume that Jupiter and his moons must have been carefully examined, I do not mean merely looked at, at least one hundred thousand times during the last hundred years.

Now we shall be able to understand the extraordinary interest which the announcement of the detection of a fifth satellite has created. Here was this system which everyone knew, which had occupied so much attention, and

now we are told on the best authority that there is something to be seen in it which had eluded all the eyes that ever looked at it before. This is indeed a surprise. Those who have good telescopes will think on the fortune which might conceivably have smiled on them, if perchance the satellite had presented itself on one of those rare occasions when it might have come within the reach of instruments less powerful than that by which it was actually discovered. The mathematical astronomers to whom the problem of Jupiter has ever been an attractive though a very difficult subject will find that the new satellite imparts an entirely fresh aspect to the question. It will now, doubtless, be attacked again with a quickened interest, and it is certain that the movements of the newly discovered body will suggest considerations of great theoretical importance. It is even quite possible that its detection may have the effect of removing some of the difficulties that have hitherto been experienced in the attempts to interpret the movements of the four older bodies.

The Lick Observatory had already become famous from the numerous valuable observations which have been made within the last few years. We certainly mean no disparagement to its previous achievements when we say that they have been altogether cast in the shade by the announcement of the last discovery which has been made on the summit of Mount Hamilton. We fully appreciate the splendid series of double-star discoveries by Burnham. We recognise the value of the observations of Mars, of the beautiful lunar photographs, of the admirable and instructive spectroscopic work of Keeler, but from henceforth it would seem that the Lick telescope must be chiefly remembered, not for these admirable labours, but as the

instrument with which Jupiter's fifth moon has been found.

In this respect the Lick telescope may be compared with another celebrated instrument of America, the great Washington refractor. Doubtless much excellent work has been done by this latter instrument, besides that achievement by which its name will be specially handed down. It was with this superb glass at Washington that Professor Asaph Hall discovered the two satellites of Mars in 1877. This at once raised the name of Hall to a high rank in the list of famous astronomical discoverers. Now we have a triumph of the same high order won with the Lick telescope. This entitles the name of Barnard to be inscribed on the same select roll as that which contains the name of Asaph Hall.

On such an occasion astronomers of all countries freely offer their hearty congratulations to those who pursue their science in America. In no other country can there be found such a lavish and splendid endowment of astronomical observatories. Nowhere else is there such abundant provision for the carrying on of astronomical work of all kinds. It were fitting that the rewards should go to the credit of the country which has done most to earn them. There is no civilised nation whose inhabitants would not have experienced a thrill of pride if such a discovery as that of the two moons of Mars or of the fifth satellite of Jupiter had been achieved within its borders by one of its own people. As it happens, both these distinctions belong to America, and those who are fully acquainted with the matter know how valiantly the American astronomers have struggled with their difficulties and how trium-

phantly they have overcome them. Nor should it be forgotten in this connection that the great Lick telescope as well as the Washington telescope is of American manufacture. Both are the products of the consummate optical skill of Messrs. Alvan Clark, of Massachusetts. Those who provided these grand instruments, those who made them, those who used them, and the nation which owns them, are all to be sincerely congratulated on the splendid results of their joint efforts.

The orbit of Jupiter so nearly resembles a circle that the distance from the earth to the planet does not greatly alter. Accordingly there is not much variation in the distance from the earth to the planet at one opposition and another. It does so happen that in the opposition through which Jupiter passed in 1892, the actual distance attained was almost the smallest possible. Even that, however, is nearly four times as great as the distance from the earth to the sun. In the case of Jupiter, the most important question, so far as the advantages for observation are concerned, is the season of the year when the opposition takes place. For observations of a planet it is specially desirable to have the body as high as possible in the heavens. The atmospheric difficulties, which are always so embarrassing to the astronomer, are lessened with every increase of the altitude. This consideration will show how the opposition referred to offered exceptionally favourable advantages for the observation of Jupiter. As the great planets move in planes which are nearly coincident with the ecliptic, it follows that the best time for observing the planet will be during the winter season. Of course, the most suitable moment, so far as altitude is concerned, would be when the oppo-

sition took place on midwinter day, while the lowest altitude would be reached in an opposition on Midsummer Day. It is true that the ideally perfect opposition was not reached in 1892. The opposition took place in October, that is to say, two months before the most suitable time. But on the whole the conditions were unusually favourable.

Professor E. E. Barnard had already obtained deserved fame as a skilful astronomical observer, and therefore it is that his announcement of this new discovery has been at once accepted by astronomers. It is the extreme minuteness of the body which is the cause of its having hitherto escaped notice. We are told that the fifth satellite appears as a star of the thirteenth magnitude, if not even very much less. An object possessing no greater brilliance than is thus indicated can only be perceived by a good telescope under the most favourable circumstances. When, however, the difficulties of seeing the satellite are enhanced by the fact that it is located close to so brilliant a globe as the great planet, then it is only the exceptional powers of the Lick telescope, and the exceptional excellence of the situation in which the telescope is placed, which have enabled it to be detected at all. So far as we can estimate the lustre of the new satellite, it can hardly be the five-hundredth part of the lustre of even the faintest of its older companions in the same system. Indeed not improbably the proportion must be expressed by a figure considerably greater than that which I have written. If one of the older satellites were crushed into a thousand equal fragments, the bulk of one of these fragments would be comparable with that of the new satellite.

The distance of this new moon from the centre of the planet appears to be about 112,500 miles, and the period of each revolution is about 11 hours 57 minutes. It will thus be noticed that the satellite revolves round Jupiter in a period which exceeds that required by Jupiter to accomplish a rotation on his own axis, namely, 9 hours $55\frac{1}{2}$ minutes. The new satellite is so close to the surface of Jupiter that the difficulty of this detection is greatly enhanced by the fact that it is so frequently hidden by the great globe. Only for a comparatively small part of each revolution does the little body appear well away from the margin of the planet. When most remote it will be at a distance of 36 seconds from the edge, that is, about two-thirds of the diameter of Jupiter. Then six hours later it will be at a similar distance on the opposite side of its orbit. It is often difficult to observe one of the large satellites when in the act of transit across the planet's disc, so that we can hardly be surprised that the transits of an object which is such an extremely small fraction of their size should not be perceived.

Of course, there is a notable difference between the case of a transit of a satellite over its primary and that of a planet, like Venus or Mercury, in front of the sun's disc. In the latter case the planet appears as a black spot against the brilliant background. In fact, it may be remembered that an unsuccessful search for an intra-Mercurial planet has actually been conducted in the manner thus suggested by seeing if it could not be observed during the progress of the transit. But the case is very different when a satellite of Jupiter transits over the face of the planet. The lustre of the satellite,

arising as it does from sunbeams only, is equal to the lustre of the face of the planet, except in so far as inequalities in the intrinsic reflecting powers of the two bodies may suffice to cause a difference. The shadow of the new satellite on the globe of the planet, though no doubt it would be an extremely small point, would still nevertheless be intensely black in comparison with the surrounding surface, and therefore it might be expected that it ought to be comparatively easy to see when sufficient optical power was available. It must, however, be observed that the diameter of this shadow is considerably less than the tenth of a second, and therefore far too minute to be recognisable as a dark spot. As there is but little variation in Jupiter's distance from the sun, it will be almost equally well displayed at every opposition, if not to observatories in the British Islands, then to observatories elsewhere. Thus, for instance, if the opposition happened to be in June, as will sometimes occur, then, though the planet will be very low down for observers in our latitudes, yet it will be very favourably placed for astronomers in the southern hemisphere. Thus we may hope that we shall speedily accumulate a considerable quantity of observations relative to the new object.

To realise all that is implied by this discovery of an additional moon to the four previously known members of Jupiter's system, it will be necessary to refer to another point. Every one who knows anything of astronomy is aware that the distances of the several satellites from the centre of the planet, and the periodic times in which they revolve, are connected by a definite relation. This is, of course, an immediate inference from Kepler's famous law.

It may be worth while to refer to this, as it leads to the recognition of certain important facts in connection with the new discovery. Let us consider, for instance, the innermost of the four well-known satellites of Jupiter. It revolves round its primary in a period which, according to the best determination, may be taken as 1 day, 18 hours, 27 minutes, 34 seconds. We may regard this orbit as circular and the distance of the satellite from the centre of Jupiter as 262,000 miles. In like manner the outermost of the four satellites revolves around Jupiter in a period of 16 days, 16 hours, 32 minutes, 11 seconds, and the length of its radius is 1,170,000 miles. There is a certain relation between the four magnitudes I have named, which is expressed by saying that the squares of the times are proportional to the cubes of the distances. As this law depends upon gravitation it must be obeyed by any new satellite, and here we can foresee that the Barnard moon, whatever else it may do, must at all events revolve in an orbit under such conditions that the cube of its radius bears to the square of the periodic time the same relation as in the case of each of the other satellites.

In estimating the distance of a satellite from its primary, the most natural unit of measurement to adopt is not to be expressed in miles or in thousands of miles. It should rather be given in terms of the equatorial radius of the planet. The sense of proportion is gratified in this way of looking at the matter. This is specially advantageous in the case of Jupiter's moons, and we shall proceed to illustrate it by pointing out the movements that would be appropriate for moons placed at different distances from the centre of Jupiter. The critical case of a moon which

was as close as possible to the surface of the planet so as just to graze it, is one of peculiar interest. The moon so circumstanced would have to hurry round its vast circle in something less than three hours. If its pace fell short of that the body would fall into the planet. Were it greater, then the body would fly off into a different path altogether. If a satellite were situated at a distance from Jupiter exactly equal to the radius of the planet at its equator, then the time of revolution would be just a little more than eight hours. With every increase in the distance there is a corresponding increase in the period. I need not follow the matter into any further detail beyond stating that if the distance of the satellite were ten times the radius of the planet, then the periodic time would be about 92 hours.

There is, however, one special case of so much interest that it must not be passed over. We have hitherto said nothing as to the rotation of Jupiter on its axis. Were Jupiter a rigid body throughout its mass, and did it contain neither oceans nor an atmosphere, then the speed at which the planet rotates would have no significance so far as the movements of the satellites were concerned. But, of course, the supposition just made is anything but correct with regard to the constitution of Jupiter. It is doubtful if there are any parts of its vast globe which could be described as rigid, while it is certain that it is enveloped by a prodigious atmosphere. In other words, Jupiter is composed of materials which are liable to tidal influence. This being so, the speed of Jupiter's rotation on his axis is a very important element in the consideration of the movement of his satellites. We may take the period of rotation at 9 hours $55\frac{1}{2}$ minutes. This

would be the periodic time of a satellite which was situated at a distance above the surface of the planet which was about one and a quarter times as much as his radius; more accurately, this magnitude would be 2·273 equatorial radii of the planet distant from the centre of Jupiter. A satellite which revolved in this critical orbit would occupy quite an exceptional position, as the time of its revolution would equal that of the planet's rotation, so that the satellite would be constantly over the same spot of the planet. The planet would, in fact, always bear the same face towards an observer situated on the satellite, just as the moon always turns the same face to the earth. It is, however, certain that the new satellite is somewhat outside this critical position.

The fact that the period of revolution of the new satellite happens to be so near twelve hours leads to a somewhat singular difficulty in determining its movements. It is plain that to find the orbit which it pursues with any precision it would be desirable to combine observations made when it was now at one side and now at the other side of Jupiter. Let us suppose that at midnight the satellite is at its greatest distance to the east of the planet. It can then, of course, be observed under the most favourable circumstances. Six hours later the satellite, having accomplished half a revolution, will be at the greatest elongation west; but then Jupiter will be so placed that it cannot be observed. Daylight, of course, follows, and in twenty-four hours after the first observation the satellite will have resumed the position which it had at midnight the preceding night. It thus follows that at one observatory it can only be possible for many nights in succession to observe the satellite at one of the sides of Jupiter. If

the period of revolution had happened to be an hour or two different from what it actually is, then there would have been no such difficulty. A few nights after one elongation had been observed the other would have been presented in a convenient phase for measurement. As it is, however, Professor Barnard must have his observations supplemented by those at some other observatory in a considerably different longitude. Here is the difficulty. Telescopes of sufficient power to show the new object there may be, but the exceptionally favourable conditions for observation presented on the top of Mount Hamilton are not easily to be paralleled elsewhere. It is, however, generally found that once an object has been discovered it is frequently possible to observe it again with telescopic advantages greatly inferior to those with which the discovery was made.

As to the physical character of the new satellite it seems difficult to offer any surmise. It seems probable that so small an object must be in the solid state. We are in the habit of accounting for the obviously non-solid condition of Jupiter himself by the excessive heat which he still contains. But the new satellite bears to Jupiter a proportion, let us say, of a grain of mustard-seed to a cocoa-nut. It therefore appears that though such a vast bulk as Jupiter may not yet have had time to cool down into the solid form, the same can hardly be averred of its tiny companion. If there should be any fluid materials on the small satellite they must be distracted by the most terrific tides. It is certain that seas on its surface would be submitted to tidal forces at least thirty times as potent as those on the earth.

There cannot be a doubt that Barnard's discovery will

afford much occupation for mathematicians. It is hardly possible that it will not be the means of lending a fresh impetus to the study of the entire Jovian system. At the same time we must remember that the detection of the new body does not offer to us, so far as we are at present advised, any interesting information of the same character as that which the satellites of Mars presented. The mass of Mars was an element not very confidently known until the satellites had been discovered, and their distances and periods measured. The mass of Jupiter, however, is one of the most carefully determined elements of the solar system. It has been accurately ascertained by the movements of its satellites, especially of the fourth. There can hardly be a doubt that the value assigned to it is right to within one-thousandth part of the whole. We have, therefore, nothing further of this kind to expect from the new satellite.

It will also be remembered that one of the most astonishing features of the Martian system of moons was the extraordinarily rapid motion of the inner of the two, by which it coursed three times round the planet before the planet turned round once. This fact at the time of its announcement was unique in the whole solar system. There was never a case known before in which a secondary planet accomplished a revolution in less time than the primary accomplished a rotation. And so far as this discovery in regard to Jupiter goes, the peculiar feature of the inner moon of Mars still remains unique. Even though the period of the satellite is only about twelve hours, it is still about two hours longer than the time which Jupiter requires to spin round his axis. We ought, however, here to notice that the rotation of

Jupiter is exceptionally rapid. If the great planet required as much time for one of his rotations as does Mars, or the earth, then the new satellite of Jupiter would present the same anomalous feature to its primary as we actually find in Mars and its inner satellite.

There are so many mysteries about Jupiter that we are specially glad to welcome the new satellite in the hope that somehow it will afford a clue to their explanation. We are still ignorant of the true nature of the great red spot which has a period of rotation different from that of the planet itself. Then there are the small black spots which appear to revolve round the planet in a few minutes less time than the planet itself requires for its revolution. Professor Barnard's discovery is one of those achievements which is often succeeded by others of the same class. This has, indeed, been the case. The Fifth Satellite has been followed by a Sixth and a Seventh which have been detected by Photography. Professor Perrine, at the Lick Observatory, has the high honour to have added to our knowledge of the Solar system by the detection of these delicate objects.

CHAPTER VI.

MARS.

THE facts with regard to the opposition of Mars in 1892 are sufficiently noteworthy to be stated with some detail, and we may first set them forth even at the risk of repeating a few things that will be familiar to those who have diligently studied the Nautical Almanack. It appears that the orbit of this particular planet is especially remarkable, among planetary orbits generally, for its departure from the circular form so nearly assumed in the movements of most of the other similar bodies. Mars has an orbit of so much eccentricity that its distance from the sun varies very considerably. It is sometimes as much as 153,000,000 miles off. It is sometimes as little as 127,000,000 miles. The orbit in which our earth revolves is much more nearly circular than is the orbit of Mars, but still the variations of the distance between the earth and the sun are too large to be overlooked, even though they may seem relatively unimportant. Under certain circumstances our earth may be as far from the sun as 93,500,000 miles, while the smallest magnitude to which the distance can shrink is 90,500,000 miles. These

few facts will enable us to estimate the stretch of space that divides us from the other world in which so much interest is now being taken.

The longest distance that could possibly intervene between the two globes is found when the sun lies between them and when they are each at their greatest possible distance from it. On the other hand the most favourable condition for the observation of Mars will be when the planet is making its nearest approach to the sun, and when the earth happens to be in the same direction as Mars from

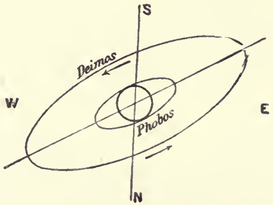

Fig. 13.—Orbits of the Satellites of Mars.

the sun. It can be shown that the very lowest value which the planet's distance from the earth can possibly assume would be about 35,000,000 miles. Nor is the condition of things which we have supposed one which will be often realised. No doubt every two years and two months, or more accurately every 780 days, the sun and Mars and the earth come nearly into a straight line, the earth being between the other two bodies ; whenever this happens we have what is called the opposition of Mars. If the orbits of both Mars and the earth were circular, then any one opposition would be as good as any other, so far as proximity is concerned : for the distance between the earth and

the planet on each such occasion would be simply the difference between their two distances from the sun. But, as we have already seen, the orbits are not circular, and consequently there is very considerable variety in the different oppositions as regards the advantages which they offer to the astronomer. It might, for instance, happen that Mars was at its greatest distance from the sun at the time when the earth crossed between it and the sun. Then the interval between the two bodies would be more than 60,000,000 miles, and the opposition would be as unsuitable as it could possibly be. It thus follows that such a very favourable opposition as that through which Mars passed in 1892 only arises from a particular combination of circumstances which but rarely occur.

It may, however, be of interest to lay down the principles which exhibit the law by which the succession of such oppositions is determined. The opposition of Mars can occur while the earth is at any part of its orbit; that is, the opposition may happen in any month of the year. The part of Mars' path which at present lies nearest the sun runs towards that part of the earth's track through which the earth passes in August. Hence it follows that if an opposition takes places in August it does so at a time when Mars is as near to the sun as is possible. It is true that the earth is not then nearest to the sun, but as the effect contributed by the variation of the earth's distance is of little importance, it follows for all practical purposes that when the opposition takes place in August, it does so under the most desirable circumstances.

On the other hand, if it should happen that the opposition took place about February, then the conditions would be as unfavourable as possible, for though Mars, earth, and sun were in a straight line in the order I have named,

yet at this part of its path Mars is at its greatest distance from the sun, and consequently the opposition takes place

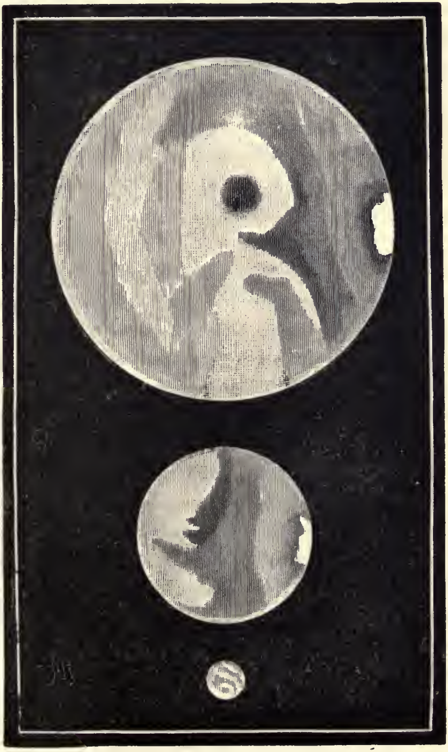

Fig. 14.—Apparent dimensions of Mars at its mean and extreme distances. At the greatest distance Mars, being on the other side of the Sun, is invisible.

when the two bodies are separated by the greatest distance that is at present possible on the occasion of an opposition. It thus happens that in the February opposi-

tions the distance between the two bodies is double as great as it is in the August oppositions. At double the distance the planet only looks one-fourth the size, and hence the appearance of Mars, when the opposition is in February, is widely different from that which it presents in the glories of an August opposition.

We can now understand why such an opportunity as that which we are at present referring to is a rare one. In the first place an opposition of Mars occurs once every 780 days. In the second place the opposition is just as likely in the long run to take place in one month as another. Only, however, when it occurs about August is it a really favourable one. If a friend paid us a visit once every two or three years, and if his visits were impartially distributed over the various seasons, it would not be on many occasions in a lifetime that we should expect to receive him during the grouse shooting. Of somewhat similar infrequency are the favourable visits of Mars, but whenever he does happen to come into opposition about the time when the grouse are being slaughtered, then his ruddy form blazes with an unwonted splendour.

A knowledge of these facts points out that the opposition of Mars in 1892 was the best that has offered itself since 1877, and the best that will offer itself for many years to come. Hence it is that so much interest has been manifested in this phenomenon, for though it would not be true to say that Mars is our nearest neighbour in the heavenly host, yet there are circumstances which render his globe much more instructive to us than any of the other heavenly bodies.

Of course, the moon is always much closer to the earth than is Mars. Even when the moon is at its greatest

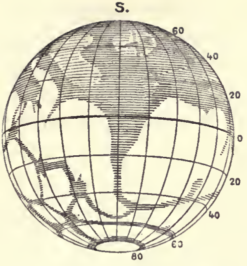

Fig. 15.—The Seasons in Mars.
May 27, 1890.

distance from us it is still not one-hundredth part of the distance by which we are divided from Mars when that planet is at its nearest. Yet we can never look on the moon as a neighbouring world in the same sense in which we look at Mars. The moon is a globe of quite a different order from the earth. Its want of air and water in any measure comparable with the abundance of such elements on the earth at once establishes so profound a difference between it and the earth, that we naturally relinquish the supposition that our satellite can have any resemblance whatever to the earth when considered as the abode of organized life.

But there is another planet with which, in all probability, we have much closer affinities than we have even with Mars. The planet Venus happens to be almost exactly of the same size as the earth. If models of the two globes were inspected, it would require careful measurement to say

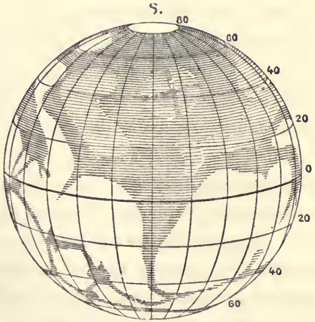

Fig. 16.—The Seasons in Mars.
August 4, 1892.

which of the two was the greater, though, as a matter of fact, to some insignificant extent, we may remark that both in volume and in mass the earth exceeds the sister planet. Venus is also, in a strict sense, a closer neighbour to us than Mars. At no time can it wander so far from us as Mars is accustomed to do, while at its closest approach the distance from Venus to the earth is less than two-thirds of that by which Mars when nearest still remains separated from us. Nor are other points of resemblance between the earth and Venus wanting. Especially may we notice that, like its companion globe, Venus is encompassed with a copious atmosphere. Everything, therefore, so far as we can judge, points to the conclusion that Venus is a world resembling our own in important features of physical constitution, so that quite possibly it is adapted to be a residence for organized beings.

Here, unfortunately, telescopic examination gives us but little aid. Notwithstanding the considerable size of Venus, and the closeness with which she makes her approach, we are unable to scrutinize her surface with the success that we desire. That very splendour which makes the evening star so lustrous an object decks the planet in such a shining robe that we are unable to make out the details on its surface. We can, no doubt, sometimes see that her form is an exquisite crescent which passes through a succession of phases. We can occasionally detect, under rare favourable circumstances of climate and instrumental equipment, slight indications or marks on the surface of the planet which, with some help from the imagination, we can suppose to be indications of continents. Then, again, some observers have noticed that

in the "cusps" at the ends of the crescent occasional interruptions and irregularities are presented which have been interprcted as implying the existence of great mountains on Venus. But when this is admitted we have said almost all that has ever been alleged to be discernible by us of the topography of that globe which is really our nearest planetary neighbour. The little that we have seen merely suggests what a wonderful spectacle might be disclosed could we put Venus into a more favourable position. If Venus were placed where Mars is, then the greater size of the former planet would make it a far more striking spectacle than Mars ever can be. Mars happens to be the more interesting globe to us simply because it is better placed for observing. Everybody knows that you can read your book comfortably if you sit with the light so nearly behind you that it may fall on the page at which you are looking. This is the aspect in which Mars is presented at opposition. The sun. which illuminates Mars, is then at midnight, behind the observer, but its beams are directed full on the planet, and exhibit it under the most favourable conditions. But Venus is presented to us in quite a different manner. It is not pleasant to try to read with the lamp in front of you, and your book held up between you and the lamp. Yet this is the way we have to look at Venus when it makes its closest approach. The consequence is that, while astronomers have abundance to tell us about the appearance of Mars, they have but little to say about the features of that other globe which is both larger and nearer to us than Mars, and with which, in all probability, we have closer affinities than we have with any other body in the universe.

From one cause or another, it happens that Mars is the

most world-like of all the other globes which come within the range of effective observation. It would, indeed, be very rash to assert that other bodies may not have a closer resemblance to our earth than Mars has, but of them we have either little knowledge, as in the case of Venus, or no knowledge at all. No doubt both Jupiter and Saturn can vie with Mars in the copiousness of detail with which they delight the astronomers who study them. These grand planets are deserving of every attention, but then the interest they excite is of a wholly different kind from that which makes a view of Mars so attractive. Jupiter offers us a meteorological study of the most astounding cloud-system in creation. Saturn gives an illustration of a marvellous dynamical-system the like of which would never have been thought possible had it not actually presented itself to our notice. But the significance of Mars is essentially derived from those points of resemblance to the earth which are now engrossing attention. Mars is clearly a possible world, presenting both remarkable analogies and remarkable contrasts to our own world, and inducing us to put forth our utmost endeavours to utilise so exceptional an occasion as that presented in the close approach which it made in 1892. Let us see what we have learned about this globe.

In the first place, it should be noticed that Mars must be a small world in comparison with our own. The width of this globe is only 4,200 miles, so that its volume is but the seventh part of that of the earth. The weight of Mars is even less than what might have been expected from his bulk. It would take nearly ten globes, each as heavy as Mars, to form a weight equal to that of the earth. This fundamental difference in dimensions be-

tween Mars and our globe is intimately connected with certain points of contrast which it offers to the earth.

Of these the most important is that which concerns the atmosphere. When we consider what the qualifications for a globe as a possible abode for organic beings should be, it is natural to inquire first into the presence or the absence of an atmosphere. Seeing that our earth is enveloped by a copious covering of air, it follows that the beings which

Fig. 17.—Mars and one of its Satellites.

dwell upon its surface must be specially adapted to the conditions which the atmosphere imposes. Most, if not all, animals utilise this circumstance by obtaining a proximate source of energy in the union of oxygen from the atmosphere with oxidizable materials within their bodies. In this respect the atmosphere is of such fundamental importance that it is difficult for us to imagine what that type of life best adapted for existence on an airless globe would be. In other respects which are hardly less important, the conditions of life are also dependent on the fact that we live at the bottom of an

ocean of air. It is the atmosphere which, to a large extent, mitigates the fierceness with which the sun's rays would beat down on the globe if it were devoid of such protection. Again, at night, the atmospheric covering serves to screen us from the cold that would otherwise be the consequence of unrestricted radiation from the earth into space. It is, therefore, obvious that the absence of a copious atmosphere, though perhaps not so absolutely incompatible with life of some kind, must still necessitate types of life of a wholly different character from those with which we are familiar. In attempting, therefore, to form an estimate of the probability of life on another world, it is of essential importance to consider whether it possesses an atmosphere.

We may here lay down a canon which appears to be pretty general among the celestial bodies accessible to our observations. It may be thus stated. The larger the body the more abundant the atmosphere by which that body is surrounded. Of course this rule has to be understood with certain qualifications, and perhaps some exceptions to it might be suggested, but as a broad general fact it will hardly be questioned. Thus, to take at once the largest body of our system and one of the smallest—the sun and the moon—they both provide striking exemplifications of the principle in question. It is well known that the sun is enveloped by an atmosphere remarkable for the prodigious extent that it occupies. On the other hand the moon, which is by far the smallest of the bodies readily accessible to our observations, is, if not entirely devoid of gaseous investment, at all events only provided with the scantiest covering of this nature. But the chief interest that the principle we have laid

down possesses, is found in the explanation which has been given of it. That explanation is both so recent and so remarkable that I am glad here to have the opportunity of setting it forth, as it has an important application in the case of Mars.

Modern research has demonstrated that what we call a gas is in truth a mighty host of molecules far too small to be perceptible by the most powerful microscope. Each of these molecules is animated by a rapid movement, which is only pursued for a short distance in one direction before an encounter takes place with some other molecule, in consequence of which the directions and velocities of the individual molecules are continually changing. For each gas the molecules have, however, a certain average pace, which is appropriate to that gas for that temperature, and when two or more gases are blended, as in our atmosphere, then each molecule of the constituent gases continues to move with its own particular speed. Thus, in the case of the air, the molecules of oxygen as well as the molecules of nitrogen, are each animated by their characteristic velocity, and the same may be said of the molecules of carbonic acid or of any other gas which, in more or less abundance, may happen to be diffused through our air. For two of the chief gases the average velocities of the molecules are as follows: oxygen, a quarter of a mile per second; hydrogen, one mile per second; in each case the temperature is taken to be 64° C. below zero, being presumably that at the confines of the atmosphere. It will be noticed that there is a remarkable difference between the speeds of the two molecules here mentioned. That of hydrogen is by far the greatest of any gas.

We may now recall a fundamental fact in connection with any celestial body large or small. It is well known that, with the most powerful pieces of artillery that can be forged, a projectile can be launched with a speed of about half a mile per second. If the cannon were pointed vertically upwards the projectile would soar to a great elevation, but its speed would gradually abate, the summit of its journey would be duly reached, after which it would fall back again on the earth. Such would undoubtedly be the case if the experiment were made on a globe resembling our own in size and mass. But on a globe much smaller than the earth, not larger, for instance, than are some of the minor planets, it is certain that a projectile shot aloft from a great Armstrong gun would go up and up and would never return. The lessening gravitation of the planet would fail to recall it. Of course we are here reminded of Jules Verne's famous projectile. According to that philosopher, if a cannon was pointed vertically, and the projectile was discharged with a speed of seven miles a second it would soar aloft, and whether it went to the moon or not, it would at all events not return to the earth except by such a marvellous series of coincidences as those which he has described. But the story will at all events serve to illustrate the fact that for each particular globe there is a certain speed with which if a body leaves the globe it will not return.

It is a singular fact that hydrogen in the free state is absent from our atmosphere. Doubtless many explanations of a chemical nature might be offered, but the argument employed by Dr. G. J. Stoney is most interesting, inasmuch as it shows that the continued existence of hydrogen in our atmosphere would seem to be impossible.

No doubt the average speed at which the molecules of this gas are hurrying about is only one mile a second,

Fig. 18.—Comparative sizes of Earth, Mars, Mercury, and the Moon.

and, therefore, only a seventh of the critical velocity required to project a missile from the earth so as not to

return. But the molecules are continually changing their velocity and may sometimes attain a speed which is seven times as great as the average. Suppose, therefore, that a certain quantity of hydrogen were diffused through our air, every now and then a molecule of hydrogen in its wanderings would attain the upper limit of our atmosphere, and then it would occasionally happen that with its proper speed it would cross out into space beyond the region in which its movements would be interfered with by the collisions between other atmospheric molecules. If the attraction of the earth was sufficient to recall it, then, of course, it would duly fall back, and in the case of the more sluggishly moving atmospheric gases the velocity seems always small enough to permit the recall to be made. But it happens in the case of hydrogen that the velocity with which its molecules are occasionally animated rises beyond the speed which can be controlled by terrestrial gravity. The consequence is that every now and then a molecule of hydrogen succeeds in bolting away from the earth altogether, and escaping into open space. Thus it appears that every molecule of free hydrogen which happened to be present in an atmosphere like ours, would have an unstable connection with the earth, for wherever in the vicissitudes of things it happened to reach the very uppermost strata it would be liable to escape altogether. In the course of countless ages it would thus come to pass that the particles of hydrogen would all effect their departure, and thus the fact that there is at present no free hydrogen in the air over our heads may be accounted for.

If the mass of the earth were very much larger than it is, then the velocities with which the molecules of hydro-

gen wend their way would never be sufficiently high to enable them to quit the earth altogether, and consequently we might in such a case expect to find our atmosphere largely charged with hydrogen. Considering the vast abundance of hydrogen in the universe, it seems highly probable that its absence from our air is simply due to the circumstances we have mentioned. In the case of a globe so mighty as the sun, the attraction which it exercises, even at the uppermost layers of its atmosphere, is so intense that the molecules of hydrogen never attain pace enough to enable them to escape. Their velocity would have to be much greater than it ever can be if they could dart away from the sun as they have done from the earth. It is not, therefore, surprising to find hydrogen in the solar atmosphere. In a similar manner we can explain the abundance of hydrogen with which the atmospheres of other massive suns like Sirius or Vega seem to be charged. The attraction of these vast globes is sufficiently potent to retain even an atmosphere solely composed of this subtle element.

It is now easy to account for the absence of atmosphere from the moon. We may feel confident from the line of reasoning here followed that neither of the gases, oxygen or nitrogen, to say nothing of hydrogen, could possibly exist in the free state on a globe of the mass and dimensions of our satellite. The pace with which the molecules of oxygen and nitrogen speed on their way would be quite sufficient to render their abode unstable if it should ever have appeared that circumstances placed such gases on the moon. We need, therefore, feel no surprise at the absence of any atmosphere from the neighbouring globe. The explanation is given by the laws of dyna-

mics. We are placed at too great a distance from the small planets or asteroids, as they are called, to be able to see whether or not they have any gaseous surroundings. But it is possible, from the ingenious argument we are discussing, to assure ourselves that such small bodies must be quite as devoid of air as the moon. There are, we know, globes in our system only a few miles in diameter, and so small in mass, that a cricket-ball there, receiving the velocity it would get from the bat of a W. G. Grace, would go off into space never to return. It is quite obvious that the molecules of any gases we know would be far too nimble in their movements to remain prisoners at the surface of little globes of this description, to which their only bond was the feeble attraction of gravitation. It is, therefore, in the highest degree improbable—we might, indeed, almost say impossible—for gaseous surroundings to be preserved by any globe where the mass is not considerably greater than that of the moon.

In applying these considerations to Mars we have first to note that its mass and size are intermediate between those of the earth and the moon. It is much more capable of retaining an atmosphere than the moon, though its capability in this respect falls short of that possessed by the earth. But in such a case it is essential to depend not on mere generalities but on the actual numerical facts of the case. Without going too deeply into detail it is sufficient to observe that there must be for each globe a certain critical velocity represented by the least pace at which a missile projected from it will succeed in escaping altogether. In discussing this we may leave out of view the question of the resistance which the air opposes to

the passage of the projectile. This is, no doubt, of vital importance in cases where actual artillery practice is concerned, yet it is not material to our present inquiry. The problem which we are considering depends on the movements of the molecules of air at the summit of the atmosphere, and the question is simply whether after they have made an incursion into free space there is sufficient efficiency in the attraction of the globe to effect their recall.

At the surface of Mars the speed which would carry a body away from its surface altogether is about three miles per second. It seems certain that the velocity of the molecules of hydrogen is often far in excess of this, and consequently free hydrogen is impossible as a permanent ingredient of the Martian atmosphere. Oxygen, however, has a molecular velocity only about one-fourth of that of hydrogen, and it seems unlikely that the oxygen molecules can ever have sufficient velocity to permit their escape from an atmosphere surrounding Mars. There is nothing, therefore, to prevent this element from being now present.

But the case of the vapour of water is especially instructive and interesting. Its molecules have a speed which averages about one-third of that attained by the molecules of hydrogen. It would seem that the utmost pace that the molecules of water could attain (being perhaps seven times the average velocity) would be about $2\frac{1}{3}$ miles per second. Now this would not be enough for escape from Mars, for we have seen that a speed of three miles per second would be required for this purpose. This argument suggests that the globe of Mars happens to approach very closely the dimensions and mass of the

smallest world on which the continued existence of water would be possible. It would perhaps be going rather too far to say that a world almost the size of Mars must therefore be the smallest on which life could possibly be supported, but it is plain that our argument tends to support such a proposition.

The discussion we have just given will prepare us to

Fig. 19.—Mars through the telescope.

believe that a planet with the size and mass of Mars may be expected to be encompassed with an atmosphere. Our telescopic observations completely bear this out. It is perfectly certain that there is a certain shell of gaseous material investing Mars. This is shown in various ways. We note the gradual obscuration of objects on the planet as they approach the edge of the disc, where they are necessarily viewed through a greatly increased thickness

of Martian atmosphere. We also observe the clearness with which objects are exhibited at the centre of the disc of Mars, and though this may be in some measure due to the absence of distortion from the effects of foreshortening, it undoubtedly arises to some extent from the fact that objects in this position are viewed through a comparatively small thickness of the atmosphere enveloping the planet.

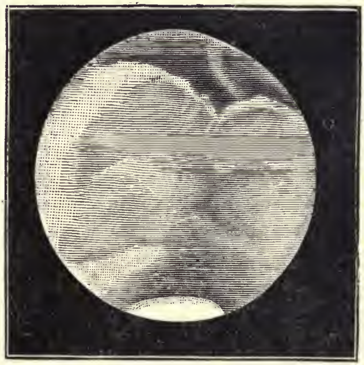

Fig. 20.—Mars through the telescope.

Clouds are also sometimes seen apparently floating in the upper region of Mars. This, of course, is only possible on the supposition that there must be an atmosphere which formed the vehicle by which clouds were borne along. It is, however, quite obvious that the extent of the Martian atmosphere must be quite insignificant when compared with that by which our earth is enveloped. It is a rare circumstance for any of the main topographical

features, such as the outlines of its so-called continents or coasts of its so-called seas, to be obscured by clouds to an extent which is appreciable except by very refined observations.

Quite otherwise would be the appearance which our globe would present to any observer who should view it say from Mars, or from some other external world at the same distance. The greater part of our globe would seem swathed with vast clouds through which only occasional peeps could be had at the actual configuration of its surface. I dare say a Martian astronomer who had an observatory with sufficiently good optical appliances, and who possessed sufficient patience, might in the course of time, and by availing himself of every opportunity, gradually limn out a chart of the earth which would in some degree represent that with which we are familiar in our atlases. It would, however, be a very tedious matter owing to the interruptions to the survey caused by the obscurities in our atmosphere. The distant astronomer would never be able to comprehend the whole of our earth's features in a bird's-eye glance as we are able to do those features on that hemisphere of Mars which happens to be turned towards us on a clear night.

As to what the composition of the atmosphere on Mars may be we can say but little. In so far as the sustenance of life is concerned, the main question, of course, turns on the presence or the absence of oxygen. It may be pertinent to this inquiry to remark here that a globe surrounded by air may at one epoch of its career have free oxygen as an ingredient in its atmosphere, while at other epochs free oxygen may be absent. This may arise from another cause besides the possible loss of the gas by

diffusion into space from small globes in the manner already explained. Indeed, it seems quite probable that the oxygen in our own air is not destined for ever to remain there. It passes through various vicissitudes by being absorbed by animals and then restored again in a free state under the influence of vegetation. But there is an appetite for oxygen among the inorganic materials of our globe which seems capable of using up all the oxygen on the globe and still remaining unsatiated. We have excellent grounds for believing that there is in the interior of the earth a quantity of metallic iron quite sufficient to unite with all the free oxygen of the air so as to form iron oxide. In view of the eagerness with which oxygen and iron unite, and the permanence of the compound which they form, it is impossible for us to regard the presence of oxygen in the air as representing a stable condition of things. It follows that, even though there may now be no free oxygen in the atmosphere of Mars, it is by no means certain that this element has always been absent. It is, however, not at all beyond the reach of scientific resources to determine what the actual composition and extent of the atmosphere of Mars may be, though it can hardly be said that as yet we are in full possession of the truth.

An almost equally important question is as to the telescopic evidence of the presence of water on Mars. Here, again, we have to be reminded of the fact that even when the planet makes its closest approach it is still actually a very long way off. It would be impossible for us to say with certainty that a region which by its colour and general appearance looked like an ocean of water was really water or was even a fluid at all. It is

so easy to exaggerate the capabilities of our great telescopes that it may be well to recount what is the very utmost that could be expected from even our greatest instrument when applied to the study of Mars. Let us consider, for example, the capabilities of the Lick telescope in aiding such an inquiry as that before us. This instrument, both from its position and its optical excellence, offers a better view of Mars at the present time than can be obtained elsewhere. But the utmost that this telescope can perform in the way of rendering remote objects visible is to reduce the apparent distance of the object to about one-thousandth part of its actual amount. Some, indeed, might consider that even the Lick instrument would not be capable of giving so great an accession to our powers as this statement expresses. However, I am willing to leave the figure at this amount, only remembering that if I estimated the powers of the telescope less highly than these facts convey, the argument on which I am entering would be correspondingly strengthened.

As we have already said, Mars is at a distance of 35,000,000 miles during a favourable opposition, and if we look at it through a telescope of such a power as we have described the apparent distance is reduced to one-thousandth part. In other words, all that the best telescope can possibly do is to exhibit the planet to us as it would be seen by the unaided eye if it were brought within a distance of 35,000 miles. This will demonstrate that even our greatest telescopes cannot be expected to enable us to answer the questions that are so often asked about our neighbouring globe. What could we learn of Europe if we had only a bird's-eye view of it from a height of

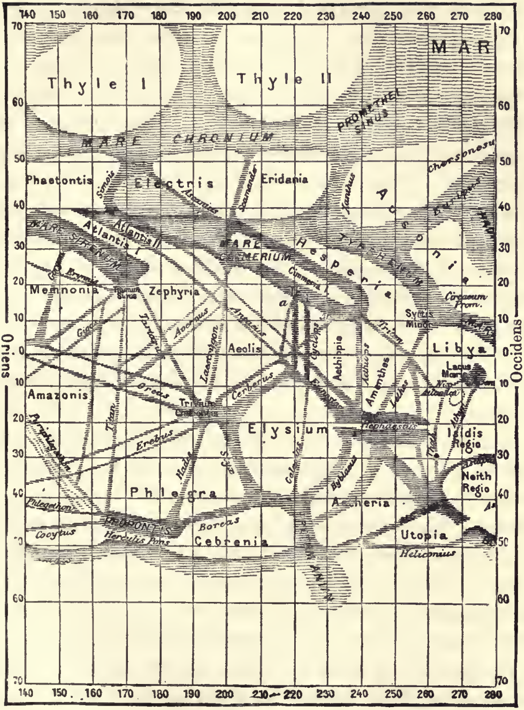

Fig. 21.—Part of a Map of Mars.

35,000 miles, that is to say, from a height which was a dozen times as far as from the shores of Europe to America? The broad outlines of the coast might, of course, be seen by the contrast between the colour of a

continent and the colour of the ocean. Possibly a great mountain mass like the Alps would be sufficiently noticeable to permit some conjectures as to its character to be formed. But it is obvious that it would be hopeless to expect to see details. The smallest object that would be discernible on Mars must be as large as London. It would not be possible to see a point so small as either Liverpool or Manchester would be if they were on that planet. There is no doubt a remarkable contrast between the dark colours of certain parts of Mars and the ruddy colours of other parts. We would, however, be unwarranted in asserting that the former must be oceans of water, and the latter continents of land. This may indeed be the case, and most astronomers, I believe, think that it is the case, but it certainly has not yet been proved to be so.

Undoubtedly the most striking piece of evidence that can be adduced in favour of the supposition that there is water on Mars is derived from the "snowy" poles on the planet. The appearance of the poles on Mars with their white caps is one of the most curious features of the solar system. The resemblance to the structure of our own polar regions is extremely instructive. It is evident that there must be some white material which from time to time gathers in mighty volume round the north and south poles of the planet. It is also to be noticed that this accumulation is not permanent. The amount of it waxes and wanes in correspondence with the variations of the seasons on Mars. It increases at either pole of Mars during the winter of that pole. In this respect the white regions, whatever they may be composed of, present a noteworthy contrast to the majority of the

other features on the planet. The latter offeı no periodic changes to our notice; they are evidently comparatively permanent marks, not to any appreciable extent subject to seasonal variations. When we reflect that this white material is something which grows and then disappears according to a regular period, it is impossible to resist the supposition that it must be snow, or possibly the congealed form of some liquid other than water, which during Mars' summer is restored to a fluid state. There can hardly be a doubt that if we were ever able to take a bird's-eye view of our own earth its poles would exhibit white masses like those which are exhibited by Mars, and the periodic fluctuations at different seasons would produce changes just like those which are actually seen on Mars. It seems only reasonable to infer that we have in Mars a repetition of the terrestrial phenomenon of arctic regions on a somewhat reduced scale

Among the features presented by Mars there are others, in addition to the polar caps, which seem to suggest the existence of water. It was in September, 1877, when Mars was placed in the same advantageous position for observation that it subsequently occupied in 1892, that a remarkable discovery was made by Professor Schiaparelli, the director of the Milan Observatory. In the clear atmosphere and the convenient latitude of the locality of his observatory, he was so fortunate as to observe marks not readily discernible under the less advantageous conditions in which our observatories are placed. Up to this time it was no doubt well known that the surface of Mars could be mapped out into districts marked with more or less distinctness, so much so that charts of the planet had been carefully drawn and names had been assigned to the

various regions which could be detected with sufficient certainty. But at the memorable opposition to which we have referred, the distinguished Italian astronomer discovered that the tracts generally described as "continents" on Mars were traversed by long, dark "canals," as he called them. They must have been each at least sixty miles wide, and in some cases they were thousands of miles in length. Notwithstanding the dimensions to which these figures correspond, the detection of the Martian canals indicates one of the utmost refinements of astronomical observation. The fact that they are so difficult to see may be taken as an illustration of what I have already said as to the hopelessness of discerning any object on this planet unless it be of colossal dimensions.

It is impossible to doubt that considerable changes must be in progress on the surface of Mars. It is true that, viewed from the distance at which we are placed, the extent of the changes, though intrinsically vast, seem relatively insignificant. There is, however, too much testimony as to the changes to allow of hesitation. As an illustration of what is meant, we may refer to the subsequent observations of the canals made by Schiaparelli, their discoverer. During the opposition of 1881 and 1882, he again recognised the presence of these curious objects, but it would seem that a very extraordinary transformation had taken place in some of them. They had become doubled. In certain cases a pair of canals could be detected, separated by an interval of two hundred miles or more, and running parallel to each other throughout their whole length. Again, in the opposition of 1888, other astronomers, notably Dr. Terby and M. Perrotin, have also made observations confirming the remark-

Fig. 22.—Canals on Mars observed by MM. Perrotin and Thollon, 1886.

able phenomenon of the duplicity in the canals. Professor Schiaparelli has, on the same occasion, confirmed his previous observations, and, notwithstanding that the opposition of 1888 was not really an advantageous one, yet under exceptionally favourable circumstances, he declares that he saw the hemisphere of Mars so exquisitely delineated that the canals had all the distinctness of an engraving on steel, with the magical beauty of a coloured painting.

Speculations have naturally been made as to the explanation of these wonderful canals. It has been suggested that they may indeed be rivers; but it hardly seems likely that the drainage of continents on so small a globe as Mars would require an elaborate system of rivers each sixty miles wide and thousands of miles in length. There is, however, a more fatal objection to the river theory, in the fact that the marks we are trying to interpret sometimes cross a Martian continent from ocean to ocean, while on other occasions they seem to intersect each other. Such phenomena are, of course, well-nigh impossible if these so-called canals were in any respect analogous to the rivers which we know on our own globe. It can, however, hardly be doubted that if we assume the dark regions to be oceans the canals do really represent some extension of the waters of these oceans into the continental masses. Other facts which are known about the planet suggest that what seem to be vast inundations of its continents must occasionally take place. Nor is it surprising that such vicissitudes should occur on a globe circumstanced like Mars. Here again it is well to remember the small size of the planet, from which we may infer that it has progressed through its physical

evolution at a rate more rapid than would be possible with a larger globe like the earth. The sea is constantly wearing down the land, but by upheavals arising from the intensely heated condition of the interior of our globe the land is still able to maintain itself above water. It can, however, hardly be doubted, that if our earth had so far cooled that the upheavals had either ceased or were greatly reduced, the water would greatly encroach on the land. On a small globe like Mars the cooling of the interior has so far advanced that, in all probability, the internal heat is no longer an effective agent for indirectly resisting the advance of the water, and, consequently, the observed submergence is quite to be expected.

That there may be types of life on Mars of some kind or other is, I should think, very likely. Two of the elements, carbon and hydrogen, which are most intimately associated with the phenomena of life here, appear to be among the most widely distributed elements throughout the universe, and their presence on Mars is in the highest degree probable. But what course the progress of evolution may have taken on such a globe as Mars, it seems totally impossible to conjecture. It has been sometimes thought that the ruddy colour of the planet may be due to vegetation of some peculiar hue, and there is certainly no impossibility in the conception that vast forests of some such trees as copper-beeches might impart to continental masses hues not unlike those which come from Mars. Speculations have also been made as to the possibility of there being intelligent inhabitants on this planet, and I do not see how anyone can deny the possibility at all events of such a fact. I would suggest, however, that as our earth has only been tenanted by intelligent beings

for an extremely brief part of its entire history, say, for example, for about one-thousandth-part of the entire number of years during which our globe has had an independent existence, so we may fairly conjecture that the occupancy of any other world by intelligent beings might be only a very minute fraction in the span of the planet's history. It would, therefore, be highly improbable, to say the least of it, that in two worlds, so profoundly different in many respects as are this earth and Mars, the periods of occupancy by intelligent beings should happen to be contemporaneous. I should therefore judge that, though there may once have been, or though there may yet be, intelligent life on Mars, the laws of probability pronounce against the supposition that there is such life there at this moment.

We have also heard surmises as to the possibility of the communication of inter-planetary signals between the earth and Mars, but the suggestion is a preposterous one. Seeing that a canal, sixty miles wide and a thousand miles long, is an object only to be discerned on exceptional occasions, and under most favourable circumstances, what possibility could there be that, even if there were inhabitants on Mars who desired to signal to this earth, they could ever succeed in doing so? We are accustomed to see ships signalling by flags, but what would have to be the size of the flags by which the earth could signal to Mars, or Mars signal to the earth? To be effective for such purpose each of the flags should be, at least, as big as Ireland. It is true, no doubt, that small planets would be fitted for the residence of large beings, and large planets would be proper for small beings. The Liliputians might be sought for on a globe like

Jupiter, and the Brobdingnagians on a globe like Mars, and not *vice versâ* as might be hastily supposed. But no Brobdingnagian's arms would be mighty enough to wave the flag on Mars which we should be able to see here. No building that we could raise, even were it a hundred times more massive than the Great Pyramid, would be discernible by the Martian astronomer, even had he the keenest eyes and the most potent telescopes of which our experience has given us any conception.

CHAPTER VII.

POINTS IN SPECTROSCOPIC ASTRONOMY.

THE annual meeting of the British Association in 1891 had a peculiar interest for astronomers, inasmuch as the assembly at Cardiff was gathered together to hear an address on the subject of Modern Astronomy from the lips of one who is admittedly the founder of a great branch of astronomical physics.

There is no Englishman, there is no man of any other nation, who could speak with the same authority as Sir William Huggins on the achievements of the spectroscope in the exploration of the heavens. To realise fully what he has done we must contrast our present knowledge with the knowledge that was possessed thirty years ago. Up to the middle of the present century the progress of astronomy along the older lines had no doubt been marvellous. The discovery of Neptune had illustrated in a forcible manner the completeness of mathematical astronomy. The movements of the planets had become so thoroughly understood that, though here and there small discrepancies were recognised, yet it seemed that the difficulties remaining to be vanquished were

only akin to those which had been already overcome. More comets no doubt could be found, more minor planets were being constantly discovered, but the older methods did not supply much fresh intellectual pabulum. They provided, it is true, additional material for the application of well-known formulæ; they required the computation of tables similar in scope to scores of other tables that were already in hand. But it certainly seemed that if astronomy was to sustain the high interest that it had always possessed, some new departure was necessary in order that the science might exhibit that growth which seems to be an essential requisite of vitality. It was about thirty years ago that the much needed advance was made which opened up to research a vast department of science of a totally unexpected character.

Comte was one of those who, in alluding to the probable exhaustion of attainable astronomical science, indicated some problems which were apparently beyond the reach of our powers. We might, he surmised, find out much with regard to the movements of the heavenly bodies, we might survey their distances, measure their dimensions, and appraise their weight; but, said Comte, to find out their material composition or to learn the actual chemical elements of which they are composed, this problem, though it would be pregnant with interest for us, we could not but despair of solving. It was not many years before this rash assertion was disproved by the splendid discoveries which, to the astonishment of the world, explained the meaning of the dark lines in the solar spectrum, and demonstrated the existence of iron and other well-known metals in our great luminary.

It is essential to the right understanding of the subject to

comprehend adequately the enormous accession to our knowledge which this indicated. Chemists had studied the structure of our globe for centuries ; they had ascertained that it was composed of some sixty or seventy elements ; but they knew nothing as to the composition of the heavenly bodies. The sun, moon, and stars might, for anything we knew at that time, be composed of elements quite as unknown to us as lithium or any other rare metal was to Aristotle. The only indication of the chemical composition of bodies external to the earth was obtained from meteorites. It was, indeed, noted with interest that meteorites contained no elements except those which were already known to exist on the earth. The origin of meteorites was, however, at that time too obscure to enable any sound inference to be drawn about the composition of the celestial bodies generally. Indeed it might have been urged with much force that as the meteorites had been falling on the earth for countless ages an appreciable proportion of the materials on the earth's surface may have been accumulated from this source, so that the meteoric elements must be already discoverable in the list of terrestrial substances. In fact, we knew absolutely nothing about the composition of the globes external to the earth, and any information that was forthcoming on this subject was thus presented in the light of a revelation.

I do not here attempt to give any historical account of the discoveries. My only object is to indicate the position which Sir W. Huggins occupies, so as to comment on the address which he so fitly delivered at Cardiff. It is natural in this connection to refer to the lecture which Huggins delivered at Nottingham before the British Association in 1866. On that occasion, as some of

those who listened to him at Cardiff may perhaps remember, he described the memorable discoveries by which he extended the methods of spectrum analysis to several of the heavenly bodies. He showed the spectra which he and the late Professor Miller had already succeeded in obtaining of some of the brightest stars, notably of Aldebaran and Betelguese. He had measured the dark lines with which the spectra of these stars were crowded, and it was shown by their positions that certain well-known terrestrial substances must be present in those distant luminaries. In reference to many of these elements the coincidence is based not on one line but on several lines, so that it is impossible to shake the testimony which the spectroscope affords as to the identity, in part at all events, of the constituents of the stars with the materials in the solar system.

On referring to this memorable lecture of 1866, it is indeed surprising to find how discoveries seemed to crowd together at the commencement of Sir William Huggins' career. He had at that time noticed the characteristic spectrum presented by white stars, of which Sirius is one type, and had demonstrated the existence of hydrogen in stars of this class. He had also examined coloured stars, like Alpha Herculis, and had found them to exhibit a spectrum, in which portions of the coloured bands are subdued by strong groups of lines in such a way as to afford an explanation of the hues which these stars display. He had demonstrated in the case of Beta Cygni that sufficient lines are found in the blue and violet parts of the spectrum of the large star to make the red and yellow rays predominate, thus giving to the lustre of the larger star of this celebrated pair a hue that is often known as

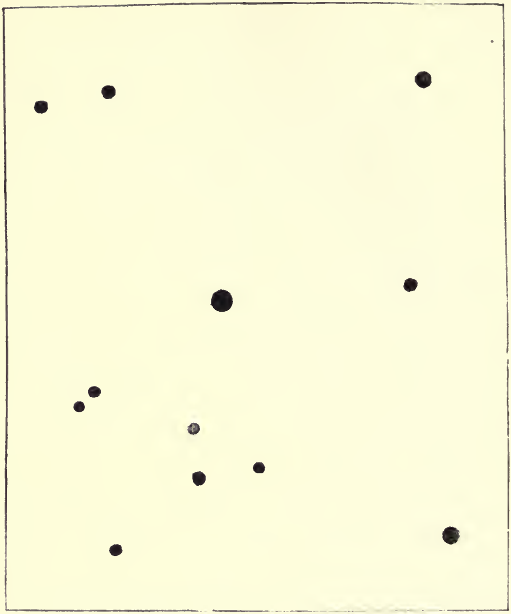

Fig. 23.—The Region of the Milky Way about β Cygni. Showing twelve bright stars.

topaz colour. On the other hand, the small and delicate blue companion shows a spectrum in which the strongest groups of lines occur in the orange, yellow, and part of the red.

There is no more pleasing phenomenon in sidereal astronomy than that presented by the contrasted hues often exhibited by double stars. It was, however, always in some degree a matter of uncertainty as to how far these varied hues were to be regarded as actually

indigenous to the stars, for it seemed not at all impossible that there might be some optical explanation of colours so vividly contrasted emanating from points so contiguous. It was also remembered that blue stars were generally only present as one member of an associated pair, and it was thought, not it must be confessed without plausibility, that the blue hue which was exhibited might have arisen from some subjective cause, or at all events that it did not necessarily imply that the star actually possessed a bluish colour. When, Sir William Huggins showed that the actual spectrum of the object demonstrated that the cause of the colour in each star arose from absorption by its peculiar atmosphere, it became impossible to doubt the reality of the phenomena. Since then it has been for physicists to explain why two closely neighbouring stars should differ so widely in their atmospheric constituents, for it can be no longer contended that their beautiful hues arise from an optical illusion.

Another achievement in the early part of Sir William Huggins' career is connected with the celebrated new star that burst forth in the Crown in 1866. It seemed a fortunate coincidence that just at the moment when the spectroscope was beginning to be applied to the sidereal heavens, a star of such marvellous character should have presented itself. I well remember going with Lord Rosse in 1866 to pay my first visit to Sir William Huggins at Tulse Hill. One of the objects he showed us was the spectrum of this star, which on the 12th of May in that year suddenly burst forth with a lustre of the second magnitude in the constellation of the Northern Crown. At the time of my visit the star had considerably declined from its original radiance. The feature which made the

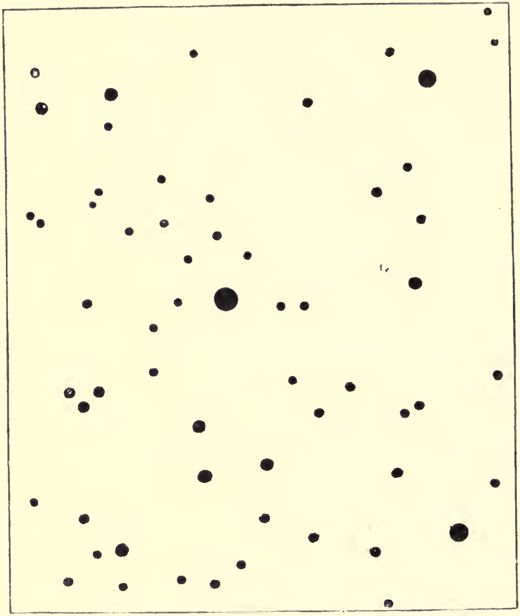

Fig. 24.—The Region of the Milky Way about β Cygni. Showing sixty stars.

spectrum of the new star essentially distinct from that of any other star that had been previously observed was the presence of certain bright lines superposed on a spectrum with dark lines of one of the ordinary types. The position of certain of these lines showed that one of the luminous gases must be hydrogen. It is impossible to dissociate the spectroscopic evidence from the circumstances known in connection with this star. The spectroscope showed that

there must have been something which we may describe as a conflagration of hydrogen on a stupendous scale, and this outburst would account for the sudden increase in luminosity of the star, and also to some extent explain how so stupendous an illumination once kindled could dwindle away in so short a time as a few days. Viewed in the light of much later work, these early discoveries assume an increased significance.

If we were to choose that one of Sir William Huggins' achievements which gave the widest extension to our knowledge, I think we can hardly hesitate to select what Romney Robinson long ago called the "palmary discovery" of the spectrum of a nebula. It was here that in the most emphatic sense Sir W. Huggins broke new ground. The stars were known to be bodies more or less congenerous with our sun; and up to the time of which I am speaking, about a quarter of a century ago, nebulæ were often looked upon as clusters of stars too distant for us to perceive the rays from each individual point. In fact, with the erection of each great telescope the test of its performance was generally sought in its power to "resolve" nebulæ, as the process used to be called. It is true that many nebulæ wholly refused to disintegrate, but it was generally, though not universally, thought that, with increased power, even the most refractory nebula would exhibit itself as a mere cloud of stars. Remembering this fact, and remembering also the faintness of these mere stains of light, it may be readily believed that when Huggins first allowed one of these objects to throw its gleam on the slit of his spectroscope, he did not entertain much hope that this instrument, though so potent elsewhere, would avail to interpret such a dim object. If

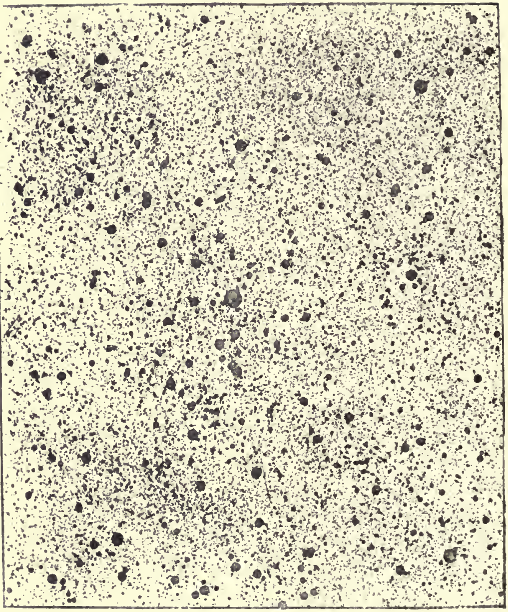

Fig. 25.—The Region of the Milky Way about β Cygni. Showing thousands of telescopic stars.

the nebula was of the same order as stars which had been observed, then its light would be expanded by the prism, and weak as the light was at the beginning it would become much weaker still when spread out in the act of dispersion. When, however, Huggins found that he could see light in the spectroscope he so little realised the nature of his important discovery that he thought for the moment what he saw must have had its origin in some

maladjustment of his apparatus. But it was not so. He discovered that the nebula he was looking at, as well as many other objects of the same class, was not a mere distant cluster of stars, but that they were masses of glowing gas.

The action of the prism on light from a star is utterly different from its action on the light emitted from glowing gas. In the former case the light is spread out into the long band displaying the rainbow hues if bright enough; in the latter case the light is condensed into one or more luminous lines. The light from the gaseous nebula is exhibited by the spectroscope in a number of bright lines instead of being spread out over the entire length of the spectrum. That nothing should be wanting to complete this splendid contribution to our knowledge of the universe Sir William Huggins discovered the nature of the gases which glow in these faint bluish nebulæ. Even at this early period he succeeded in establishing the existence of hydrogen in these remote regions of space.

The important discoveries we have named may be said to have initiated the application of spectroscopic research to the sidereal heavens. The address that Huggins delivered at Cardiff presents a splendid picture of the harvest of discoveries by this time accumulated. It is natural that so attractive a field of research should have engaged the co-operation of many zealous explorers. To their labours the address rendered ample justice.

At the present moment the attention of the astronomical world is especially directed towards the development of the resources of photography in the various applications which it has to their art. Already the camera has become

an indispensable adjunct in the observatory, and we are every day learning more and more of what it can do for us. The chemical eye is often more sensitive than the human eye; it is always more patient. It will display for us a magnificent nebula like that which surrounds the Pleiades, and which is wholly invisible to the unaided eye, and only to be seen with the telescope under very special conditions not often realised. Naturally, Huggins has discussed at length the applications of photography. It would be impossible for him not to have mentioned that photograph obtained by the late Dr. Roberts of the great nebula in Andromeda, which was produced by exposing a highly sensitive plate for four hours in the focus of a powerful reflector. The result has been to produce a picture which has been said, and I believe with truth, to be the most suggestive representation of any celestial object that has ever been obtained. Features which had been dimly traced in the nebula when visually examined in powerful telescopes are now seen to be parts of an organic whole, visible on the photograph, though not otherwise discernible by the keenest sense.

Such a study of this great nebula was all the more acceptable because it is one of the most baffling of these objects. It is bright enough to be perceived by the unaided eye, and it might have been expected that so striking a celestial structure ought by this time to have disclosed its character either as a distant cluster of stars, or as a truly gaseous object. Herschel long ago called it one of the least resolvable of the nebulæ, but yet it does not appear to possess a spectrum similar to that of the gaseous nebulæ of which we have been speaking. The character

of this object, both as to its actual physical nature and as to the materials present in it, is at present undetermined. This consideration lends a certain amount of mystery to Dr. Roberts' great photograph, a mystery we do not feel to a corresponding extent when we look at the photograph of Andromeda's only rival, the great nebula in Orion. The pictures of the latter exhibit a glorious object which is certainly known to be gaseous, and we have also the assurance that hydrogen is among the materials of which it is composed.

No part of Sir William Huggins' address was better than that which treated of the exquisite application of the spectroscope to the discovery of the movement of approach or movement of recession in the object from which the light emanates. In fact there is no passage in the address which seems to me more pregnant in significance than that in which he remarks that: "In the future a higher value may indeed be placed upon this indirect use of the spectroscope than upon its chemical revelations." As to the accuracy of this method, it enables us, under favourable circumstances, to measure speed of recession or approach "to within a mile per second, or even less." What this means is that such a speed as that of the revolution of the earth in its orbit around the sun could be determined to within five or six per cent. of its amount. It is to Sir W. Huggins himself that we are indebted for the first application of this principle to astronomical measurement. The earliest observations were made by him in 1868, but for many years the application of this method was retarded by a want of perfection in the instruments necessary for so delicate a branch of research. However, such improvements have

been made within the last two years, by means of photography, at Potsdam, and by eye observations at the Lick Observatory, that the method has been elevated to a precision that entitles its measurements to the respect which has always been accorded to those made by the appliances of the older astronomy.

Professor Vogel at Potsdam photographs a small part of the spectrum of the star in the vicinity of the line G, and for the purpose of comparison introduces with all needful precaution the hydrogen line in that neighbourhood. For certain stars he has recently used some of the lines of iron. The result we must give in Sir W. Huggins' own words. "The perfection of these spectra is shown by the large number of the lines, no fewer than 250 in the case of Capella, within the small region of the spectrum on the plate. Already the motions of about fifty stars have been measured with an accuracy, in the case of the larger number of them, of about an English mile per second."

In a method of such delicacy, involving results of so great interest, it is obviously desirable to have confirmatory measures made under circumstances as widely different as possible. These have been forthcoming from the Lick Observatory in California, thanks to the late Professor Keeler, at that great institution. He has succeeded in obtaining determinations, by direct eye observation with superb instruments, and he has found it possible to execute measurements of a spectrum with an accuracy as great as that obtained by Professor Vogel. The result is so significant that we must again give it in the words of Huggins:

"The marvellous accuracy attainable in ProfessorKeeler's

hands on a suitable star is shown by observations on three nights of the star Arcturus, the largest divergence of Keeler's measures being not greater than six-tenths of a mile per second, while the mean of three nights' work agreed with the mean of five photographic determinations of the same star at Potsdam to within one-tenth of an English mile. These are determinations of the motions of a sun so stupendously remote that even the method of parallax practically fails to fathom the depth of intervening space, and by means of light waves, which have been, according to Elkin's nominal parallax, nearly 200 years upon their journey."

It is impossible for any lover of astronomy to read of these achievements without some emotion. The alliance between photography and spectroscopy is here rendered available for extending our knowledge of the movements of the heavenly bodies in a direction wholly inaccessible to every other appliance of the astronomer. I may mention one of the points in which the importance of the new method can hardly be overrated. In the older process of ascertaining the proper motions of stars, the lapse of long periods of time was indispensable. A star would have to possess a movement more rapid than that of any of the stars, except a very few, if it could be determined by our meridian instruments in less than a twelvemonth. In the majority of cases an interval of many years would be necessary before the movement of the star could be certainly concluded from such measurements. With such small movements as those possessed by most of the stars, various causes combine to render the measurements highly uncertain; and yet for astronomers who desire to learn the constitution of the heavens, there would be no informa-

tion more valuable than copious and accurate knowledge of the proper motions of the stars. It seems from these discoveries at Potsdam and at Lick that we may now entertain a hope that abundant and accurate information of the character that I have indicated will be promptly forthcoming.

The researches of Professor Keeler at Lick have already afforded us some information with regard to the proper motions of the nebulæ in the line of sight. Here, indeed, an entirely new departure has been made. Most of these objects are so ill-defined that their position cannot be measured, or cannot by ordinary methods be even specified with the accuracy necessary for the determination of their proper motions. The vagueness of nebulæ is not, however, a bar to the application of the spectroscope in the measurement of its movements in the line of sight. We still know nothing as to the movements of nebulæ athwart that line. But it is something for us to have obtained information as to the progress of these bodies in one direction at all events. An attempt was made to solve this problem a good many years ago by Sir W. Huggins himself; but the apparatus that was then available did not possess the refinement necessary for measurements so delicate. The resources of the splendid equipment at Lick have provided what is required, and Prof. Keeler has ascertained the movements of some nebulæ. As an illustration of his results, we may take the famous nebula in Orion. He finds that this object is retreating from our system at the rate of about ten miles a second. The most rapid movement he has yet discovered in one of these nebulous objects is a pace of forty miles a second.

Among the problems which the spectroscope has yet failed to solve must be mentioned that of the Aurora Borealis. No doubt something has been learned; but still it must be confessed that the prism has been more successful up to the present in its application to objects which lie like the nebulæ on the very confines of the visible universe, than it has to the aurora, which is, comparatively speaking close at hand. Sir William Huggins gave a summary of our knowledge on this subject. It is certain that the glow of the aurora is in the main due to the effect of electric discharges in the upper regions of the atmosphere. Seeing that we are familiar with the spectra of the atmospheric gases, as produced in our laboratories, it might have been expected that the interpretation of the spectrum of the aurora would be a comparatively easy task. We are still ignorant of the source of the principal line in the green, which, as Huggins remarked, may have an origin independent of that of the other lines.

He also referred to the supposition that the aurora is produced by the dust of meteors; but with reference to this, he noted that experiment has shown that fine metallic dust suspended in gases conveying an electric discharge like that of an aurora will not cause the spectrum to exhibit the characteristic line of the metallic dust in question. There is much to be said for Professor Schuster's suggestion that the principal line in the aurora may be due to some extremely light gas which is present in too small a relative quantity in the lower strata of the atmosphere to permit of its existence being disclosed by spectroscopic or any other form of chemical analysis. In the upper regions where the auroral displays take place,

the ordinary gases have assumed extreme tenuity, and the lighter gas becomes of more relative importance, and gives a character to the spectrum.

As it is instructive to learn, as far as may be, the boundary between the known and the unknown, it is interesting to read what Sir W. Huggins has to tell us about the solar corona. The nature of this marvellous appendage to the sun is still a matter of uncertainty. There can, however, be no doubt that the corona consists of highly attenuated matter driven outwards from the sun by some repulsive force, and it is also clear that if this force be not electric it must at least be something of a very kindred character. Dr. Schuster suggests that there may be an electric connection between the sun and the planets. In fact, with some limitations we might even assert there *must* be such a connection. It is well known that great outbreaks on the sun have been immediately followed, I might almost say accompanied, by remarkable magnetic disturbances on the earth. The instances that are recorded of this connection are altogether too remarkable to be set aside as coincidences. Sir William Huggins has not referred in this connection to Hertz's astonishing discoveries; but it seems quite possible that research along this line may throw much light on the subject, at present so obscure, of the electric relation between the sun and the earth. So far as the spectrum of the corona is concerned we may summarise what is known in the words of Huggins: "The green coronal line has no known representative in terrestrial substances, nor has Schuster been able to recognise any of our elements in the other lines of the corona."

Sir William Huggins regarded it as surprising that our

first accurate knowledge of the spectrum of hydrogen should have been ascertained not from a course of refined laboratory experiments, but from photographs of the spectra of the white stars to which Sirius belongs. Hydrogen has a few visible lines in its spectrum, and the photograph shows that these belong to an organized system of lines which are wonderfully displayed in the spectra of the white stars, first fully obtained by Sir W. Huggins. The hydrogen spectrum possesses a special interest, inasmuch as Dr. Johnstone Stoney many years ago pointed out that the three principal visual lines were members of a harmonic series, and the interesting discovery has been since made by Professor Balmer that a more comprehensive law includes both these harmonic members and the rest of the series. Thus the hydrogen spectrum appears to present a simplicity not found in the spectrum of any other gas, and therefore it is with great interest that we examine the spectra of the white stars, in which the dark lines of hydrogen are usually strong and broad. In stars of this class we often look in vain for those dark metallic lines so characteristic of other stars which have a nature more nearly resembling our sun.

The question is also discussed as to whether the radiance characteristic of the white stars may be regarded as an indication of an extremely high temperature as compared with that shown by other stars. It seems hardly possible to doubt that such a star as Sirius owes its great lustre not merely to its size, but also to its intrinsic brilliancy, indicative of a high temperature. It may illustrate the attention that has been paid to the spectra of the white stars to refer to some interesting observations of Scheiner; he has found that the objects of this class which are in

the constellation of Orion agree in possessing a certain dark line, which appears to coincide in position with one of the bright lines in the famous nebula in the same constellation. He remarks that, with the single exception of Algol, he has not observed this same line in any other white star. These observations naturally suggest the remark that the stars in the constellation of Orion possess a certain affinity beyond that implied by their proximity in the same constellation. They are apparently a group associated by community of composition. In considering this circumstance we are reminded how the Great Nebula, with every increase of optical power and every increase in the period of exposure of the photographs, seems to cover an ever-widening area, extending, as we now know, so as to include several of the bright stars.

Still one more application of the spectroscopic method of measuring movements in the line of sight is found in Dunér's beautiful observations of the limb of the sun. By comparison between the approaching edge and the retreating edge he has been able to ascertain the velocity of the sun's rotation. It is not only interesting to find that these results corroborate the determinations already familiar by observations of the sun-spots, but the spectroscopic method admits of being applied to zones in the sun from which spots are absent. We thus obtain a very complete knowledge of the laws of rotation of our luminary. Dunér's measurements confirm the extraordinary fact that the equatorial regions in the sun accomplish a revolution in a shorter time than zones which are nearer to his poles.

The address of course gave some account of the progress of the combined effort to produce a great photographic

chart of the heavens. About 22,000 photographs will be necessary, each covering a space of four degrees. Each star is to appear on two plates, so as to avoid errors, and by giving an exposure of rather less than an hour it is expected that all stars down to the fourteenth magnitude will be represented. Astronomers well know how large a share of credit is due to Sir David Gill in connection with this great work. This vast surveying task is only one of the pieces of astronomical work in which photographing the stars is now employed. In the delicate movements of annual parallax it has been proved that measurements made on the photographs can compare favourably with the finest measurements made on the heavens. We are only just beginning to realise the benefits from these photographic processes.

Sir William Huggins referred to the constitution of comets and their connection with meteors. Nothing is better established than the fact that the periodic meteor shower is a swarm of minute bodies revolving around the sun in an elliptic orbit, and that in the case of some of the greater showers, at all events, the highway pursued by the meteoric shoal is also the highway in which a great comet moves. That there is a connection between comets and meteors of this periodic class seems therefore unquestionable, though it does not seem easy to say what the precise nature of the relation may be. It is, however, especially necessary to observe the distinction between the ordinary luminous meteors and the solid meteorites which occasionally tumble down on the earth. It does not seem to be at all clear that meteorites have any connection whatever with comets. The meteorites do not stand in any ascertained relation

to the periodic shooting-star showers. In fact, the only common feature which they may be said to possess is that they both come into the atmosphere from the outside. While, therefore, we must admit that such meteor showers as the Leonids are unquestionably connected with comets, yet we must distinctly hesitate to affirm that meteorites have any known relation to these bodies. On this matter Huggins has expressed himself with characteristic caution, though he acknowledged that there is some spectroscopic evidence which might be cited in support of the contention that the nucleus of the comet is not wholly different from the matter which falls down here as meteorites. With reference to the more characteristic features of comets, such as the rapid transformations which they undergo, and the marvellous tails which they shoot forth, the idea seems gradually developing that the phenomena are in the main of an electric character. Sir W. Huggins suggests that the recent discoveries of the electric action of the ultra-violet part of solar light may possibly help to explain the highly electrified condition of comets.

It would not be possible in a *résumé* of the achievements in modern astronomy to omit an account of the researches on the constitution of the sun made by the late Professor Rowland. He has shown that thirty-six terrestrial elements are certainly indicated in the solar spectrum, while eight others are doubtful. Fifteen elements have not been found though sought for, and ten elements have not yet been compared with the sun's spectrum. Reasons are also given for showing that though fifteen elements had no lines corresponding to those in the solar spectrum, yet there is but little evidence to show

that they are absent from the sun. Sir William Huggins epitomises these very interesting results in the striking remark: "It follows that if the whole earth were heated to the temperature of the sun, its spectrum would resemble very closely the solar spectrum."

The science of the last century seems destined to be famous throughout the ages. To biologists it will be the century of natural selection; to physicists it will be the century of the spectroscope.

CHAPTER VIII.

THE NEW ASTRONOMY.

ASTRONOMERS are at present endeavouring to become fully acquainted with the resources of a new tool which has recently been placed in their hands. Perhaps it would be rather more correct to say that the tool is not exactly novel in principle, but that it is rather the development of its capabilities and its application in new directions which forms the departure now creating so much interest. We have already learned much by its aid, while the expectation of further discoveries is so well founded that it is doubtful whether at any time since the invention of the telescope the prospects of the practical astronomer have seemed so bright as they are at this moment.

In the earlier periods of astronomical research it was the movements of the heavenly bodies which specially claimed attention, and it was with reference to these movements that the great classical achievements of the science have been made. But within the last two or three decades the most striking discoveries in observational astronomy have been chiefly, though by no means exclusively, concerned with the physical constitution of

the heavenly bodies. It is the application of the spectroscope by the labours of Sir W. Huggins and others that has disclosed to some extent the material elements present in the stars, as well as in comets and the distant nebulæ. Now, however, it seems as if the spectroscope were for the future to be utilised not merely for that chemical examination of objects which is in the scope of no other method, but also as a means of advancing in a particular way our knowledge of the movements of the heavenly bodies. The results already obtained are of a striking and interesting description, and it is to their exposition and development that this article is devoted.

In the first place, it will be observed that the application of the spectroscope which we are now considering is not merely to be regarded as an improvement superseding the older methods of determining the movements of stars. It is, indeed, not a little remarkable that the type of information yielded by the spectroscope is wholly distinct from that which the earlier processes were adapted to give. The new method of observing movements, and that which, for convenience, we may speak of as the telescopic method, are not, in fact, competitive contrivances for obtaining the same results. They are rather to be regarded as complementary, each being just adapted to render the kind of information that the other is incompetent to afford.

It is well known that the ordinary expression, *fixed star*, is a misnomer, for almost every star which has been observed long enough is seen to be in motion. Indeed, it is not at all likely—nay, it is infinitely improbable, that such an object as a really fixed star actually exists. When the place of a star has been accurately determined

by measurements made with the meridian circle, and when, after the lapse of a number of years the place of the same star is again determined by observation, it not unfrequently happens that the two places disagree. The explanation is, of course, that the star has moved in the interval. Thus the constellations are becoming gradually transformed by the movements of the several stars which form them. It is true that the movements are so slow that even in thousands of years the changes do not amount to much when regarded as a disturbance of the configuration. Thus, to take an example, we know the movements of the stars forming the Great Bear sufficiently well to be able to sketch the position of the stars as they were ten thousands years ago, or as they will be in ten thousand years to come, and though, no doubt, some distortion from the present lineaments of the Great Bear is shown in each of these pictures, yet the identity of the group is in each case well preserved. In Fig. 27 we have shown the amount of distortion in this constellation which would be produced in the course of 36,000 years.

It is, however, obvious that if a star should happen to be darting directly towards the observer or directly from him, the telescopic method of determining its movement becomes wholly inapplicable. No change in its position could be noticed. It is, no doubt, conceivable that if the distance of a star from the earth were determined, and if the investigation were repeated after a sufficient lapse of time, then the differences between the two distances would give an indication of the star's movement along the line of sight during the interval. But we may say at once that such a method of research is wholly impracticable. Our knowledge of the star-distances is far too imperfect

for the successful application of this method. Nor is there the slightest prospect of any improvements in practical astronomy which would enable us to detect movements of stars in the line of sight in the way suggested. Certainly it offers no hope of a method which could compare for a moment in simplicity or precision with the beautiful spectroscopic process. Of course if a star were moving in the line of sight, there must be a certain change in its apparent lustre corresponding to the changes in its distance, and it might be supposed that by careful measurements of the brightness of a star con-

Fig. 26.—The Great Bear as it is.

ducted from time to time, conclusions could be drawn as to the speed with which it was moving. But the application of such a process is beyond the sphere of available methods. It would take at least a thousand years before even the most rapidly-moving star would experience a change that would sensibly affect its lustre; and even if we had the means of measuring with precision the light emitted, our results would still be affected by the possible fluctuations in the star's intrinsic brightness.

It is thus manifest that the resources of the older astronomy were quite incapable of meeting the demands of astronomers when it became necessary to learn the move-

ments of the stars towards us or from us, as well as the movements perpendicular to the line of vision, which had always been the subject of much investigation. It is just here that the spectroscope comes in to fill the vacant place in the equipment of the astronomer. It tells exactly what the older methods were unable to tell, and it does so with a certainty that suggests vast possibilities for the spectroscopic process in the future. The principle of the method is a beautiful illustration of the extent to which the different branches of physical science are interwoven. But the principle has been a familiar one to astronomers for many years. It is the facility and success attending its recent application that has now aroused so much interest. Once it became certain that the undulatory theory of light expressed a great truth of nature, a certain deduction from that truth became almost obvious. It was, however, by no means certain that the practical application of this deduction to astronomical research would be feasible. That it has proved to be so in any degree is somewhat of a surprise, while it now appears susceptible of developments to an extent that could hardly have been dreamed of.

The logic of the new method is simple enough. Our eyes are so constituted that when a certain number of ethereal vibrations per second are received by the nerves of the retina, the brain interprets the effect to mean that a ray of, let us say, red light has entered the eye. A certain larger number of vibrations per second is similarly understood by the brain to imply the presence of blue light on the retina. Each particular hue of the spectrum —the red, orange, yellow, green, blue, indigo, violet—is associated with a corresponding number of vibrations per

second. It will thus be seen that the interpretation we put on any ray of light depends solely, as far as its hue is concerned, on the number of vibrations per second produced on the retina. Increase that number of vibrations in any way, then the hue becomes more bluish ; decrease the number of vibrations per second, and the hue shows more of the ruddy tinge.

From these considerations it is apparent that the hue of a light as interpreted by the eye will undergo modification if the source from which the light radiates is

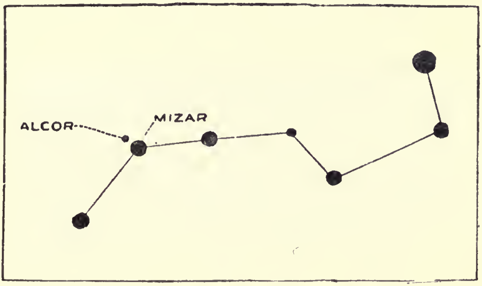

Fig. 27.—The Great Bear in 36,000 years.

moving towards us or moving from us. In order to expound the matter simply I shall suppose a case of a rather simpler type than any which we actually find in nature. Let us suppose the existence of a star emitting light of a pure green colour corresponding to a tint near the middle of the spectrum. This star pours forth each second a certain number of vibrations appropriate to its particular colour, and if the star be at rest relatively to the eye, then, we assume, the vibrations will be received on the retina at the same intervals as those with which the star emits them. Consequently we shall perceive the

star to be green. But now suppose that the star is hurrying towards us, it follows that the number of vibrations received in a second by the eye will undergo an increase. For the relative movement is the same as if the earth were rushing towards the star. In this case we advance, as it were, to meet the waves, and consequently receive them at less intervals than if we were to wait for their arrival.

Many illustrations can be given of the simple principle here involved. Suppose that a number of soldiers are walking past in single file, and that while the observer stands still twenty soldiers a minute pass him. But now let him walk in the opposite direction to the soldiers, then, if his speed be as great as theirs, he will pass forty soldiers a minute instead of twenty. If his speed were half that of the soldiers, then he would pass thirty a minute, so that in fact the speed with which the observer is moving could be determined if he counts the number of soldiers that he passes per minute, and makes a simple calculation. On the other hand, suppose that the observer walks in the same direction as the soldiers; if he maintains the same pace that they do, then it is plain that no soldiers at all will pass him while he walks. If he moves at half their rate, then ten soldiers will pass him each minute. From these considerations it will be sufficiently apparent that if the earth and the star are approaching each other, more waves of light per second will be received on the retina than if their positions are relatively stationary. But the interpretation which the brain will put on this accession to the number of waves per second is that the hue of the light is altered to some shade nearer the blue end of the spectrum. In fact, if we could conceive the velocity with

which the bodies approached to be sufficiently augmented, the colour of the star would seem to change from green to blue, from blue to indigo, from indigo to violet; while, if the pace were still further increased, it is absolutely certain that the waves would be poured upon the retina with such rapidity that no nerves there present would be competent to deal with them, and the star would actually disappear from vision. It may, however, be remarked that the velocity required to produce such a condition as we have supposed is altogether in excess of any known velocities in the celestial movements. The actual changes

Fig 28.—The Great Bear as it will be 100,000 years hence.

in hue that the movements we meet with are competent to effect are much smaller than in the case given as an illustration.

On the other hand, we may consider the original green star and the earth to be moving apart from each other. The effect of this is that the number of waves poured into the eye is lessened, and accordingly the brain interprets this to imply that the hue of the star has shifted from the green to the red end of the spectrum. If the speed with which the bodies increase their distance be sufficiently large, the green may transform into a yellow, the yellow

into an orange, the orange into a red; while a still greater velocity is, at all events, conceivable which would cause the undulations to be received with such slowness that the nature of the light could no longer be interpreted by any nerves that the eye contains, and from the mere fact of its rapid motion away from us the star would become invisible. Here again we must add the remark that the actual velocities animating the heavenly bodies are not large enough to allow of the extreme results now indicated.

However, in the actual circumstances of the celestial bodies it seems impossible that any change of hue recognisable by the eye could be attributed to movement in the line of sight. Nor does this merely depend on the circumstance that the velocities are too small to produce such an effect. It must be remembered that the case of a star which dispenses light of perfect simplicity of composition is one that can hardly exist among the heavenly bodies, though it may be admitted that there is a certain approach to it in one or two remarkable cases. It is, however, much more usual for the light from a star to be of a highly composite type, including rays not only from all parts of the visual spectrum, but also rays belonging to the ultra-violet region, as well as others beyond the extreme red end. The effect of the retreat of a star, so far as its colour is concerned, is that though the green is shifted a little towards the red, a bluish hue moves up to supply the place of the green, and as a similar effect takes place along the entire length of the spectrum, the total appearance is unaltered.

It is a fortunate circumstance that the lines in the spectrum afford a precise means of measuring the extent

of the shift due to motion. If the movement of the star be towards us, then the whole system of lines is shifted towards the blue end, whereas it moves towards the red end when the star is hastening from us. The amount of the shift is a measure of the speed of the movement. This is the consideration which brings the process within the compass of practical astronomy. We need not here discuss the appliances, optical, mechanical, and photographic, by which an unexpected degree of precision has been given to the measurements. It seems that in the skilful hands of Vogel and Keeler it is possible in favourable cases to obtain determinations of the velocities of objects in the line of sight with a degree of precision which leaves no greater margin for doubt than about five per cent. of the total amount. It is truly astounding that such a degree of accuracy should be attainable under conditions of such difficulty. It must also be remembered that the distance of the object is here immaterial, unless in so far as the reduction in the brilliancy of the star owing to its distance involves a difficulty in making the observations.

As the first illustration of the extraordinary results that are now being obtained by the application of the new process, I take the case of the celebrated variable Algol. This star is a well-known object to all star-gazers; it lies in the constellation of Perseus, and its vagaries attracted notice in early times. In ages when the stars were worshipped as divinities it is not unreasonable to suppose that a star whose light varied in any extraordinary manner should naturally be viewed with some degree of suspicion as contrasted with stars that dispense their beams with uniformity. It was doubtless a feeling of

this kind which rendered Algol a star of questionable import to the ancient students of the heavens. It was accordingly known as the Demon Star, for this is the equivalent of the name by which we now know it. As to the peculiarities of Algol which have given it notoriety, these are very simply described. For two days and ten hours the star remains of uniform lustre, being ranked about the second magnitude; then a decline of brightness sets in, and the star in a few hours parts with three-fifths of its brightness. At the lowest point it remains for about twenty minutes, and then the brilliancy commences to increase, so that in a few hours more Algol has resumed its original character. The entire period required for the decline and the rise is about ten hours, and the whole cycle of the changes has been determined with much accuracy, and is at present 2 days, 20 hours, 48 minutes, 52 seconds. The length of the period seems to undergo some trifling fluctuations of a few seconds, but on the whole the uniformity of the movement is a striking part of the phenomenon. Considering that these changes can be observed without any telescope, it is not surprising that they have been known for centuries. Indeed, it fortunately happens that there is a smaller star near Algol which serves as a convenient standard of comparison. Under ordinary circumstances Algol is much brighter than its neighbour, but when it sinks to its lowest point the two stars have almost equal lustre. It is only within the last year or two that the mystery of the variability of Algol has been at last revealed and the phenomenon of the Demon Star has received its true interpretation.

It had been suggested long ago that the loss of light

might be due to an eclipse of the brilliant star by some dark companion revolving about it; indeed, this theory seemed to hold the field, inasmuch as its only rival was one which supposed Algol to be a rotating body darker on one side than the other. This, however, was easily

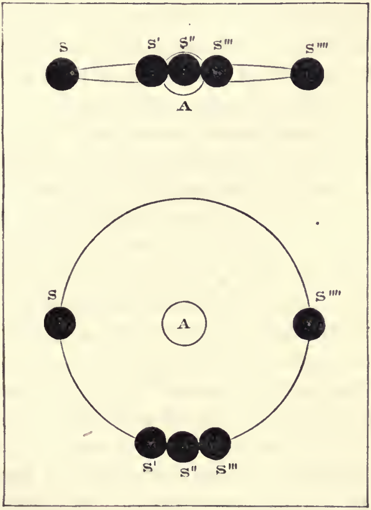

Fig. 29.—Orbit of the Companion of Algol.

shown to be incompatible with the observed facts as to the manner in which the light waxed and waned in a single cycle of change. It was, however, impossible to subject the eclipse theory to any decisive test until astronomers were provided with the means of measuring the velocity of approach or retreat along the line of sight. The existence of the dark companion was therefore almost

destitute of support from observations until Vogel made his wonderful discovery.

Applying the improved spectrographic process to Algol, he determined on one night that Algol was retreating from the earth at a speed of twenty-six miles a second. This in itself is a striking fact, but of course the velocity is not an exceptionally large one for celestial movements. We know of one star at least which moves half a dozen times as fast. When, however, Vogel came to repeat his observations he found that Algol was again moving with the same velocity, but this time the movement was towards the earth instead of from it. Here was indeed a singular circumstance demanding the careful examination which it speedily received. It appeared that the movements of Algol to and fro were strictly periodic, that is to say, for one day and ten hours the star is moving towards us, and then for a like time it moves from us, the maximum speed in each advance or retreat being that which we have mentioned, namely twenty-six miles a second. The interest awakened by this discovery culminates when it appears that this movement to and fro is directly associated in a remarkable manner with the variation of Algol's lustre. It is invariably found that every time the movement of retreat is concluded, the star loses its brilliance, and regains it again at the commencement of the return movement. It is thus plain that the changes in brilliance of the star bear an important relation to the periodic movement. Here was an important step taken.

For the next advance in this remarkable investigation we have to depend, not on our instruments, but on the laws of mechanics. We have spoken of Algol as moving to and fro, but it is necessary to observe

that it is impossible for a star to run along a straight line for a certain distance. stop, turn back, again retrace its movement, stop, and again return. Such movement is simply forbidden by the laws of motion. We can, however, easily ascertain that there is a type of motion possible for Algol which shall be compatible with the results of the spectroscopic research and also be permitted by the laws of motion. There is no objection to the supposition that Algol is moving in a path which is nearly, if not exactly, a circle. In this case it would only be moving as does the moon, or the earth, or any of the other planets. It will be only necessary to suppose that the plane of the orbit of Algol is directed nearly edgewise towards us. During the description of one semicircle Algol will be coming towards us, while, during the other semicircle it will be going from us, and thus the observed facts of the movement are reconciled with the laws of motion. Of course, this involves a certain periodic shift in the position of Algol in the heavens. It must, for instance, when moving most rapidly from us be at a distance equal to the diameter of the circle from the position which it has when moving most rapidly towards us. This is true, but the extent of the shift of place is far too small to be visible to our instruments. In fact, it can be shown that the visual size of the circle in which Algol revolves could hardly be larger than is that which the rim of a three-penny bit would appear to have if viewed from a situation five hundred miles away. It is one of the extraordinary characteristics of the spectroscopic method that it renders such an orbital movement perceptible.

The fact that Algol revolves in an orbit having been thus demonstrated, we can again call in the assistance of

the laws of dynamics to carry us a step further. Such a movement is possible on one condition and only one, and that is that there is an attracting body in the neighbourhood around which Algol revolves. Of course the student of mechanics knows that each of the bodies revolves around the common centre of gravity. The essential point to be noticed is that the spectroscopic evidence admits of no other interpretation save that there must be another mighty body in the immediate vicinity of Algol. We had already seen reason to believe in the possibility of the presence of such a companion for the Demon Star, simply from the fact of its variability. There cannot be any longer a doubt that the mystery has been solved. Algol must be attended by a companion star, which, if not absolutely as devoid of intrinsic light as the earth or the moon, is nevertheless dark relatively to Algol. Once in each period of revolution this obscure body intrudes itself between the earth and Algol, cutting off a portion of the direct light from the star and thus producing the well-known effect. Here we have such a remarkable concurrence between the facts of observation and the laws of dynamics that it is impossible to doubt the explanation they provide of the variability of this famous star.

There is, however, a further point in which the facts can be made to yield information of even a more striking character, inasmuch as it is unique of its kind. It is, of course, well known that stars in general show no appreciable discs even in our best telescopes. In fact the better the instrument the smaller does the stellar point appear. This is, of course, due to the distance at which the stars are situated. It would be easy to show that if the sun were to be viewed by an observer placed on the

nearest of the stars the apparent magnitude of its disc would be no greater than an eagle would seem if soaring overhead at an altitude three times as great as the distance of New Zealand beneath our feet. Of course, no instrument whatever would render the dimensions of such an object perceptible, though such is the delicacy of the sense of perception of light that the eye may be able to detect the radiation from a self-luminous object which is itself too small to form an image of recognisable dimensions on the retina. The stars, of course, are suns often comparable with, and often far exceeding, our own sun in lustre and dimensions, but their distance is far too large to enable us to measure their diameters by the ordinary processes of the observatory. Even if the stars were brought towards the earth so that their distances were reduced to a tenth of what they are at this moment, it does not seem at all likely that any one of them would be even then seen clearly enough to enable us to perceive its diameter.

This statement becomes the more significant when it is borne in mind that there are several cases in which, though we are not able to measure the dimensions of stars, yet we are able to weigh them. If the period of revolution of a binary star has been determined, and if the distance of the pair from the sun is also known, we then have sufficient data to enable us to compare the mass of the binary system with that of the sun. It will therefore be understood that the first observations which declare the actual dimensions of a star merit the utmost attention. They constitute a distinct and important departure in our knowledge of the universe. It is surely a noteworthy epoch in the history of astronomy when, for

the first time, we are able to apply the celestial callipers to gauge the diameter of a star. So far as surveying and measuring goes, this is the most significant piece of work in sidereal astronomy since the epoch, half a century ago, when the determination of a stellar distance first emerged from the mistiness of mere guess-work and took a respectable position among the solved problems of astronomy. Nor is our gratification at the result of Vogel's striking work lessened by the fact of its unexpectedness. Who would have predicted some few years ago that the spectroscope was to be the instrument to which we should be indebted for the means of putting a measuring tape round the girth of a star? The process and the results are alike full of interest and are of happy augury for the future.

To explain exactly how it is possible to deduce a presumable value for the diameter of Algol would lead into some technicalities that need not be here mentioned. But the principle of the method is so plain that it would be unfitting to leave it without some attempt at exposition. We are first to notice that Algol, at the moment of its greatest eclipse, has lost about three-fifths of its light: it therefore follows that the dark satellite must have covered three-fifths of the bright surface. It is also to be noticed that the period of maximum obscuration is about twenty minutes, and that we know the velocity of the bright star, which along with the period of revolution gives the magnitude of its orbit. These facts, added to our knowledge that ten hours is required for the brilliancy to sink from and regain its original lustre, enable the sizes of the two globes to be found. There is only one element of uncertainty in the matter. We have assumed that the densities of the two bodies are the same. Of

course, this may not be the case, and if it should prove to be unfounded, then some modification will have to be made in the numerical elements now provisionally assigned. There can, however, be little doubt that so far as the substantial features of the Algol system are concerned, the elements given by Vogel may be accepted.

Let us endeavour to form a conception of what Algol and its companion are like. It is worth making the attempt, because, as we have already said, Algol is the first star among "yonder hundred million spheres" of which the dimensions are approximately known. First we are to think of Algol itself. It is indeed a vast object, a glowing globe, a veritable sun, much larger than our own. The diameter of the sun would have to be increased by almost 200,000 miles to make it as great as that of Algol. But we may exhibit the relative proportions of the two bodies in a somewhat different manner. Imagine two globes, each as large as our sun; let those two be rolled into one, and we have a globe of the splendid proportions of Algol. But now for a singular circumstance which indicates the variety of types of sun which the heavens offer to our study. Though Algol is twice as big as the sun it is not twice as heavy. It is indeed an extraordinary circumstance that, notwithstanding the vast bulk of Algol, its weight is only about half that of the sun. The sun itself has a density about a fourth that of the earth, or but little more than the density of water, yet Algol has a density which is much less than that of water; in fact, this globe is apparently not much heavier than if it were made of cork. We are, of course, speaking of the average density of the star. No doubt its central portions must be dense enough, but it is impossible to resist

the conclusions that the greater part of Algol must be composed of matter in a gaseous state. Of course, such a state of things is already known to exist in many celestial bodies. The figures that have been arrived at must be regarded as subject to a possible correction, but it is difficult to repress all feelings of enthusiasm at a moment when, for the first time, so startling an extension has been given to our knowledge of the universe.

And now, as to the dark companion of Algol. Here is an object which we never have seen, and apparently never can expect to see, but yet we have been able not only to weigh it and to measure it, but also to determine its movements. It appears that the companion of Algol is about the same size as our sun, but has a mass only one-fourth as great. This indicates the existence of a globe of matter which must be largely in the gaseous state, but which, nevertheless, seems to be devoid of intrinsic luminosity. We may compare this body with the planet Saturn; of course, the latter is not nearly so large as the companion to Algol, but the two globes seem to agree fairly well as to density. As to the character of the movements of the dark companion of Algol, we can learn little, except what the laws of dynamics may teach; but the information thus acquired is founded on such well understood principles that it leaves us in no uncertainty.

It would be a natural assumption that the law of gravitation is obeyed and must be obeyed in the stellar systems. It would, indeed, be surprising if that law which regulates the movements of the bodies in the solar system should not be found to prevail in the sidereal systems also. Everything would justify us in the anticipation that this is so. Have we not learned to a large extent

the actual nature of the elementary bodies which enter iuto the composition of stars? We find that the elementary bodies in these other suns are in the main identical with those which exist in our own sun and in the earth itself. If iron attracts iron by the law of gravitation in the solar system, why should not iron attract iron in the sidereal systems as well? But we are not dependent solely on this presumption for our knowledge of the important fact that the law of gravitation is not confined to the solar system. The movements of binary stars have been studied, and it has been invariably found that the phenomena observed are compatible with the supposition that the law of gravitation prevails throughout the universe.

It would not, however, be correct to assert, as has been sometimes done, that the facts of the binary systems actually prove that gravitation is the all-compelling force there as here. The circumstances do not warrant us in expressing the matter quite so forcibly. The binary stars are so remote that the observations which we are enabled to make are wanting in the almost mathematical precision which we can give to such work when applied to the bodies of our own system. It is quite possible for mathematical ingenuity to devise a wholly arbitrary and imaginary system of force, which might explain the facts of binary stars, as far as we are able to observe them, on quite another hypothesis than the simple law that the attraction between two particles varies with the inverse square of the distance. No one, however, will be likely to doubt that it is the law of gravitation, pure and simple, which prevails in the celestial spaces, and consequently we are able to make use of it to explain the circumstances attending the movement of Algol's dark companion.

This body is the smaller of the two, and the speed with which it moves is double as great as that of Algol, so that it travels over as many miles in a second as an express train can get over in an hour. It revolves with apparent uniformity in an orbit which must be approximately circular, and it completes its journey in the brief period given above, which indicates the time of variability. So far the movements of Algol and its companion are not very dissimilar to movements in the solar system with which we are already familiar; but there is one point in which the Algol system presents features wholly without parallel in the planetary movements. It is that the two bodies are so very close together. I do not, of course, mean that they seem close by ordinary standards—for is not their distance always some three million miles? This is, however, an unusually short distance when compared with the dimensions of the two globes themselves. The dimensions of the system may be appreciated by the simple illustration of taking a shilling and a sixpence and placing them so that the distance from rim to rim is two inches. The smaller coin will represent the dark satellite and the larger one Algol, fairly correct as to position and dimensions. Viewed in this way it is evident that the dimensions of the globes bear a monstrous proportion to their distance apart when compared with the more familiar planets and satellites of our system. The tides in such a case must be of a magnitude and importance of which we have no conception from our experiences of such agencies here.

We have dwelt thus long on the subject of Algol because it was fitting to give due emphasis to the remarkable extension of our knowledge of the universe

which took place when, for the first time, we became able to measure the size of a star.

It is well known that the most difficult test-objects on which a telescope can be directed are some of those double stars of which the components have a suitable distance. If the two stars be so close together that they subtend at our system an angle not more than a few tenths of a second, then the telescopic separation of the two components is a feat to tax the powers of the most perfect instrument, and the eye of the most accomplished observer. It may, however, happen that there are double stars of which the components are much closer than this. In such a case there is not the slightest possibility of our being able to effect a visual decomposition of the pair into its components. The spectroscopic process has, however, placed at our disposal a striking method for detecting the existence of double stars, the components of which are so close that even if they were hundreds of times farther apart than they actually are they would still fall short of the necessary distance at which they must be situated before they can be separated telescopically. Indeed, we have here obtained an accession to our power so remarkable that we have not yet been able even to feel the limits within which its application must be confined.

As an illustration of this process I shall take a star which is probably as famous as Algol itself. It is Mizar, the middle star of the three which form the tail of the Great Bear (Fig. 27). Mizar has in its vicinity the small star Alcor, which is so easily seen as to make it hard for us to realise the significance of the proverb, "He can see Alcor." It is, however, possible that the lustre of Alcor may have waxed greater since ancient times. The rela-

tionship between Mizar and Alcor is closer than might be inferred from the mere fact of their contiguity on the sky. Their proximity is not an accident of situation, as is the case in some other instances when two stars happen to lie in nearly the same line of vision. The association of Alcor and Mizar is rendered highly probable from the fact that they move together in parallel directions and the same velocity. But this is the least of the circumstances that gives Mizar its interest. The star itself is a double of the easiest type, and is at the same time of striking interest and beauty. Every possessor of a telescope, large or small, knows Mizar to be one of the most suitable objects wherewith to delight the friends that visit his observatory, by a glimpse at a double star which is both easy to discern and remarkable in character. This is the second noteworthy point about Mizar; but now for the third and last, which is by far the most interesting of all, and has only lately been ascertained by a discovery which will take its place in the history of astronomy as the inauguration of a new process in the study of things sidereal.

Professor Pickering has, as is well known, been extremely successful in obtaining photographs of the spectra of the stars. Sufficient means having been placed at his disposal by Mrs. Draper, he has applied himself with remarkable results to the compilation of the Henry Draper Memorial. The photographs of the spectra of the stars that he has thus obtained exhibit a fulness of detail that some years ago could hardly have been expected even in photographs of the solar spectrum itself. Among the stars subjected to his camera was Mizar, and the photographs of the spectrum of its principal com-

ponent exhibit, as other stellar spectra do, a profusion of dark lines. These photographs being repeated at different dates, it was natural to compare them, and it was noticed that the lines sometimes appeared double and sometimes single. So striking a circumstance, of course, demanded closer investigation, and presently it appeared that this opening and closing of the lines was a periodical phenomenon. The interval between one maximum opening of the lines and the next was fifty-two days. If the star were a single object, then this phenomenon would be inexplicable. It was plain that the object could not be a single star; it must consist of a pair extremely close together, and in rapid revolution. The doubling of the lines will then be readily intelligible. When one of the components is moving towards us while the other is moving from us, all the lines belonging to one system are shifted one way, and all those belonging to the other system are shifted the other way, the effect on the spectrum being that the lines appear doubled. When the stars are moving perpendicularly to the line of sight, then their relative velocities towards the earth are equal, and the lines close up again. We thus at once learn the period of the revolution of the two components. The lines must open out twice in each circuit, and consequently we have as the first instalment of the numerical facts of the system that the period of its revolution is a hundred and four days.

It is, however, a peculiarity of the spectroscopic process that it provides us with a wealth of information on the subject. The amount by which the lines open when they separate admits of accurate measurement, and as this depends on the velocities, it follows that we obtain a deter-

mination of these velocities. It thus appears that the speed with which each of the component stars moves is about fifty miles a second. As, therefore, we know the pace at which

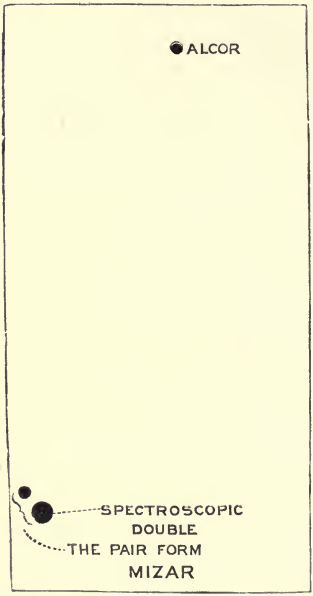

Fig. 30.—The System of Mizar.

the stars are moving, and the time they need for the journey, we know how large their path is, and thus we infer that the distance of the components is, speaking roundly, about one hundred and fifty millions of miles. But now, we are enabled to draw a remarkable inference. We

know the size of the orbits, and we know the time in which the revolutions are accomplished. It is the mathematician who enables the mass of the bodies to be determined, and the result is not a little astonishing. It tells us that the mass of the two component stars which form the primary of Mizar is not less than forty times as great as the mass of the sun. Here is indeed a result equally striking on account of the method by which it is obtained and of the startling character of the conception to which it leads.

Remember that in all this the distance of the star from the earth is not concerned, for the results at which we have arrived are absolutely independent of the distance at which the star may happen to be placed. We already knew the masses of some few binary stars by the application of the older process, but in all such cases it was necessary that we should have a previous knowledge of the star's distance. This is always a precarious element, and in the majority of cases it is wholly out of our power to discover it. Now, however, we are entitled to expect large additions to our knowledge of the stars, their masses, and their movements, notwithstanding the fact that the distances may be too vast to be appreciated by any means at our disposal.

The instances that have been given will suffice to show the versatility of the new method. It is the alliance of photography with spectroscopy that makes the present time so full of promise. Already the orbits of 150 Stars have been determined by the beautiful spectroscopic process.

CHAPTER IX.

THE BOUNDARIES OF ASTRONOMY.

IT is proposed in the following chapter to trace some parts of the boundary line which divides the truths that have been established in astronomy from those parts of the science which must be regarded as more or less hypothetical. We intend therefore to select certain prominent questions, and to discuss those questions with such fulness as the circumstances will admit.

It is desirable to commence with that great doctrine in astronomy which is often regarded as almost universally established. The doctrine to which we refer is known as the law of universal gravitation. It is customary to enunciate this law in the proposition that every particle of matter attracts every other particle with a force which varies directly as the product of the masses and inversely as the square of their distance. It is no doubt convenient to enunciate the great law in this very simple manner. It might seem awkward to have to specify all the qualifications which would be necessary if that enunciation is to assert no more than what we

absolutely know. Perhaps many people believe, or think they believe, the law to be true in its general form; yet the assertion that the law of gravitation is *universally* true is an enormous, indeed, an infinite, exaggeration of the actual extent of our information.

To make this clear, let us contrast the law of gravitation as generally stated with the proposition which asserts that the earth rotates on its axis. No one who is capable of understanding the evidence on the question can doubt that the earth really does rotate upon its axis. I purposely set aside any difficulties of a quasi-metaphysical character, and speak merely of words in their ordinary acceptation. In stating that the earth rotates upon its axis we assert merely a definite proposition as regards one body, all the facts which the assertion involves are present to our minds, and we know that the assertion must be true. Equally conclusive is the evidence for the statement that the earth revolves around the sun. Concrete truths of this kind could be multiplied indefinitely. We can make similar assertions with regard to the planets. We can assert that the planets rotate upon their axes, and that the planets revolve around the sun. But the law of gravitation is a proposition of quite a different nature. Let us examine briefly the evidence by which this law has been established.

The science of dynamics is founded upon certain principles known as the laws of motion. The simplest of these principles asserts that a body once set moving in a straight line will continue to move on uniformly for ever in the same straight line, unless some force be permitted to act upon that body. For nature as we know it, this law seems to be fully proved. It has been tested in every

way that we have been able to devise. All these tests have tended to confirm that law. The law is therefore believed to be true, at all events throughout the regions of space accessible to us and to our telescopes.

Assuming this law and the other principles analogous to it, we can apply them to the case of the revolution of the earth around the sun. As the earth is not moving in a straight line, it must be acted upon by some force. It can be shown that this force must be directed towards the sun. It will further appear that the intensity of this force will vary inversely as the square of the distance between the earth and the sun. The movements of the planets can be made to yield the same conclusions. All these movements can be accounted for on the supposition that each planet is attracted by the sun with a force which varies directly as the product of the masses, and inversely as the square of the distance between the two bodies. When more careful observations are introduced it is seen that the planets exhibit some slight deviations from the movements which they would have were each planet only acted upon by the attraction of the sun. These deviations do not invalidate the principle of attraction. They have been shown to arise from the mutual attractions of the planets themselves. Each of the planets is thus seen to attract each of the other planets. The intensity of this attraction between any pair of the planets is proportional to the masses of these planets and varies inversely as the square of the distance between them. We may use similar language with regard to the satellites by which so many of the planets are attended. Each satellite revolves around its primary. The movements of each satellite are mainly due to the preponderating attraction of the

primary. Irregularities in the movements of the satellites are well known to astronomers, but these irregularities can be accounted for by the attraction of other bodies of the system.

The law of attraction thus seems to prevail among the small bodies of the system as well as among the large bodies. It is true that there are still a few outstanding discrepancies which cannot yet be said to have been completely accounted for by the principle of gravitation. This is probably due to the difficulties of the subject. The calculations which are involved are among the most difficult on which the mind of man has ever been engaged. We may practically assume that the law of gravitation is universal between the sun, the planets, and the satellites; and we may suppose that the few difficulties still outstanding will be finally cleared away, as has been the case with so many other seeming discrepancies.

But even when these admissions have been made, are we in a position to assert that the law of gravitation is universal throughout the solar system? We are here confronted with a very celebrated difficulty. Do those erratic objects known as comets acknowledge the law of gravitation? There can be no doubt that in one sense the comets do obey the law of gravitation in a most signal and emphatic manner. A comet usually moves in an orbit of very great eccentricity; and it is one of the most remarkable triumphs of Newton's discovery, that we were by its means able to render account of how the movements of a comet could be produced by the attraction of the sun. As a whole, the comet is very amenable to gravitation, but what are we to say as to the tails of comets, which

certainly do not appear to follow the law of universal gravitation? The tails of comets, so far from being attracted towards the sun, seem actually to be repelled from the sun. Nor is even this an adequate statement of the case. The repulsive force by which the tails of the comets are driven from the sun is sometimes a very much more intense force than the attraction of gravitation.

I have no intention to discuss here the vexed question as to the origin of the tails of comets. I do not now inquire whether the repulsion by which the tail is produced be due to the intense radiation from the sun, or to electricity, or to some other agent. It is sufficient for our present purpose to note that, even if the tails of comets do gravitate towards the sun, the attraction is obscured by a more powerful repulsive force.

The solar system is a very small object when viewed in comparison with the dimensions of the sidereal system. The planets form a group nestled up closely around the sun. This little group is separated from its nearest visible neighbours in space by the most appalling distances. A vessel in the middle of the Atlantic Ocean is not more completely isolated from the shores of Europe and America than is our solar system from the stars and other bodies which surround it in space. Our knowledge of gravitation has been almost entirely obtained from the study of the bodies in the solar system. Let us inquire what can be ascertained as to the existence of this law in other parts of the universe. Newton knew nothing of the existence of the law of gravitation beyond the confines of the solar system. A little more is known now.

Our actual knowledge of the existence of gravitation in the celestial spaces outside the solar system depends

exclusively upon those interesting objects known as binary stars. There are in the heavens many cases of two stars occurring quite close together. A well-known instance is presented in the star Epsilon Lyræ, where two pairs of stars are such near neighbours that it is a fair test of good vision to be able to separate them. But there are many cases in which the two stars are so close together that they cannot be seen separately without the aid of a telescope. We may take, for instance, the very celebrated double star Castor, well known as one of the Twins. Viewed by the unaided eye, the two stars look like a single star, but in a moderately good telescope it is seen that the object is really two separate stars quite close together. The question now comes as to whether the propinquity of the two stars is apparent or real. It might be explained by the supposition that the two stars are indeed close together compared with the distance by which they are separated from us; or it could be equally explained by supposing that the two stars, though really far apart, yet appear so nearly in the same line of vision that when projected on the surface of the heavens they seem close together. It cannot be doubted that in the case of many of the double stars, especially those in which the components appear tolerably distinct, the propinquity is only apparent, and arises from the two stars being near the same line of vision. But it is, also, undoubtedly true that in the case of very many of the double stars, especially among those belonging to the class which includes Castor, the two stars are really at about the same distance from us, and, therefore, as compared with that distance, they are really close together.

Among the splendid achievements of Sir W. Herschel,

one of the greatest was his discovery of the movements of the binary stars. It was shown by Herschel that in some of the double stars one star of the pair was moving around the other, and that their apparent distances were changing. The discoveries inaugurated by Herschel have been widely extended by other astronomers. One of the more rapidly moving of the double stars lies in the constellation of Coma Berenices. The revolution of one component around the other requires a period of 25·7 years. The two bodies forming this composite star are very close together, the greatest distance being about one second of arc. There is very great difficulty in making accurate measurements of a double star of which the components are so close. More reliance may consequently be placed upon the determination of the orbits of other binary stars of which the components are further apart. Among these we may mention a remarkable binary star in Ursæ Majoris. The distance between the two components of this star varies from one second of arc to three seconds. The first recorded measurement of this object was by Sir W. Herschel in 1781, and since that date it has been repeatedly observed. From a comparison of all the measurements which have been made it appears that the periodic time of the revolution of one of these components around the other is about sixty years. This star has thus been followed through more than one entire revolution.

The importance of these discoveries became manifest when an attempt was made to explain the movements. It was soon shown that the movements of the stars were such as could be explained if the two stars attracted each other in conformity with the law of gravitation. It would, however, be hardly correct to assert that the discovery of the

binary stars proved that the two stars attracted each other with a force which varies inversely as the square of their distance. Even under the most favourable circumstances the observations are very difficult; they cannot be made with the same accuracy as is attained in observing the movements of the planets; they have not even the value which antiquity will often confer on an observation which has not much else in its favour. There are probably many different suppositions which would explain all that has yet been observed as to the motions of the binary stars. Gravitation is but one of those suppositions. Gravitation will no doubt carry with it the prestige acquired by its success in explaining phenomena in the solar system. I do not know that any one has ever seriously put forward any other explanation except gravitation to account for the movements of the binary stars, nor is any one likely to do so while gravitation can continue to render an account of the observed facts; but all this is very different from saying that the discovery of the binary stars has *proved* that the law of gravitation extends to the stellar regions.

Except for what the binary stars tell us, we should know nothing as to the existence or the non-existence of the law of gravitation beyond the confines of the solar system. Does Sirius, for instance, attract the pole star? We really do not know. Nor can we ever expect to know. If Sirius and the pole star do attract each other, and if the law of their attraction be the same as the law of attraction in the solar system, it will then be easy to show that the effect of this attraction is so minute that it would be entirely outside the range of our instruments even to detect it. Observation in such a matter is hopeless. If

we cannot detect any attraction between a star in one constellation and a star in another, no more can we detect any attraction between our sun and the stars. Such attractions may exist, or they may not exist: we have no means of knowing. Should any one assert that there is absolutely no gravitation between two bodies more than a billion miles apart there are no facts by which he can be contradicted.

If we know so little about the existence of gravitation in the space accessible to our telescopes, what are we to say of those distant regions of space to which our view can never penetrate? Let a vast sphere be described of such mighty dimensions that it embraces not only all the objects visible to the unaided eye, not only all the objects visible in our most powerful telescopes, but even every object that the most fertile imagination can conceive, what relation must this stupendous sphere bear to the whole of space? The mighty sphere can only be an infinitely small part of space. Are we then entitled to assert that every particle in the universe attracts every other particle with a force which is proportional to the product of their masses, and which varies inversely as the square of their distance? We have, indeed, but a slender basis of fact on which to rest a proposition so universal. Let us attempt to enunciate the law of gravitation so as to commit ourselves to no assertion not absolutely proved. The statement would then run somewhat as follows:—

Of the whole contents of space we know nothing except within that infinitely small region which contains the bodies visible in our telescopes. Nor can we assert that gravitation pervades the entire of even this infinitely small region. It is true that in one very minute part

of this infinitely small region the law of gravitation appears to reign supreme. This minute part is of course the solar system. There are also a few binary stars in this infinitely small region whose movements would admit of being explained by gravitation, though as yet they can hardly be held absolutely to prove its existence.

It must then be admitted that when the law of gravitation is spoken of as being universal, we are using language infinitely more general than the facts absolutely warrant. At the present moment we only know that gravitation exists to a very small extent in a certain indefinitely small portion of space. Our knowledge would have to be enormously increased before we could assert that gravitation was in operation throughout this very limited region; and even when we have proved this, we should only have made an infinitesimal advance to a proof that gravitation is absolutely universal.

I do not for a moment assert that our ordinary statement of the law of gravitation is untrue. I merely say that it has not been proved, and we may also add that it does not seem as if it ever could be proved. Most people who have considered the matter will probably believe that gravitation is universal. Nor is this belief unnatural. If we set aside comets' tails, and perhaps one or two other slightly doubtful matters, we may assert that we always find the law of gravitation to be true whenever we have an opportunity of testing it. These opportunities are very limited, so that we have but very slender supports for the induction that gravitation is universal. But it must be admitted that an hypothesis which has practically borne every test that can be applied has very strong grounds for our acceptance: such, then, are the claims of

the law of gravitation to be admitted to a place among the laws of Nature.

The series of spectroscopic researches by which Sir William Huggins has so vastly extended our knowledge should be referred to. Sir William Huggins has shown that many of the substances most abundant on earth are widely spread through the universe. Take, for instance, the metal iron and the gas hydrogen. We can detect the existence of these elements in objects enormously distant. Both iron and hydrogen exist in many stars, and hydrogen has been shown, in all probability, to be an important constituent of the nebulæ. That the rest of the sidereal system should thus be composed of materials known to be to a large extent identical with the materials in the solar system is a presumption in favour of the universality of gravitation.

In what has hitherto been said, we have attempted to give an outline of the facts so far as they are certainly known to us. Into mere speculations we have no desire to enter. We may, however, sketch out a brief chapter in modern sidereal astronomy, which seems to throw a ray of light into the constituents of the vast abyss of space which lies beyond the range of our telescopes. The ray of light is no doubt but a feeble one, but we must take whatever information we can obtain, even though it may fall far short of that which an intellectual curiosity will desire. The question now before us may be simply stated. Are we entitled to suppose that the part of the universe accessible to our telescopes is fairly typical of the other parts of the universe, or are we to believe that the system we know is altogether exceptional; that there are stars in other parts quite unlike our stars, composed of different

materials, acted upon by different laws, of which we have no conception? The presumption is that the materials of which our system is composed are representative of the materials elsewhere. This presumption is strengthened by the very important considerations now to be adduced.

In the first place, let us distinctly understand what is meant by our sidereal system. We have already dwelt on the isolated position of the sun and the attendant planets. The grandest truth in the whole of astronomy is that which asserts that our sun is only a star separated by the most gigantic distances from the other stars around. Our sun, indeed, appears to be but one of the vast host of stars which form the Milky Way. We need not here enter into the often discussed question as to whether the nebulæ are, generally speaking, at distances of the same order as the stars. There seems to be no doubt that some of the nebulæ are quite as near to us as some of the stars. At all events, for our present purpose, we may group the Milky Way, the nebulæ, the stars, and the clusters, all into one whole which we call our sidereal system. Is this sidereal system as thus defined an isolated object in space? Are its members all so bound together by the law of universal gravitation that each body, whatever be its movements, can only describe a certain path such that it can never depart finally from the system? This is a question of no small importance. It presents features analogous to certain very interesting problems in biology which the labours of Mr. Wallace have done so much to elucidate. We are told that the fauna and flora of an oceanic island, cut off from the perpetual immigration of new forms, often assume a very remarkable type. The evolution of life under such circumstances proceeds

in a very different manner to the corresponding evolution in an equal area of land which is connected with the great continental masses. Is our sidereal system to be regarded as an oceanic island in space, or is it in such connection with the systems in other parts of space as might lead us to infer that the various systems had a common character?

The evidence seems to show that the stars in our system are probably not permanently associated together, but that in the course of time some stars enter our system and other stars leave it, in such a manner as to suggest that the bodies visible to us are fairly typical of the general contents of the universe. The strongest evidence that can be presented on this subject is met with in the peculiar circumstances of one particular star. The star in question is known as number 1830 of Groombridge's catalogue. It is a small star, not to be seen without the aid of a telescope. This star is endowed with a very large proper motion. It would not be correct to say that its proper motion exceeds that of any other known star, but it certainly has the largest visible proper motion of any star of which the distance is known. The proper motion of 1830 Groombridge amounts to over seven seconds annually. It would take between two and three centuries to move over about eighteen hundred seconds, a distance in the heavens equal to the apparent diameter of the moon. The distance of this star is much greater than might have been anticipated from its very large proper motion. The estimates of the distance present some irregularities, but we shall probably be quite correct in assuming that the distance is not less than two hundred billions of miles. This star is indeed ten times as far from us as Alpha Centauri, which is generally

considered to be the sun's nearest neighbour in our sidereal system.

The proper motion and the distance of 1830 Groombridge being both assumed, it is easy to calculate the velocity with which the star must be moving. The velocity is indeed stupendous and worthy of a majestic sun; it is no less than 200 miles a second. It would seem that the velocity may even be much larger than this. The proper motion of the star which we see is merely the true proper motion of the star foreshortened by projection on the surface of the heavens. In adopting 200 miles a second as the velocity of 1830 Groombridge, we therefore make a most moderate assumption, which may and probably does fall considerably short of the truth. But even with this very moderate assumption, it will be easy to show that 1830 Groombridge seems in all probability to be merely travelling through our system, and not permanently attached thereto.

The star sweeps along through our system with this stupendous velocity. Now there can be no doubt that if the star were permanently to retain this velocity, it would in the course of time travel right across our system, and after leaving our system would retreat into the depths of infinite space. Is there any power adequate to recall this star from the voyage to infinity? We know of none, unless it be the attraction of the stars or other bodies of our sidereal system. It therefore becomes a matter of calculation to determine whether the attraction of all the material bodies of our sidereal system could be adequate, even with universal gravitation, to recall a body which seems bent on leaving that system with a velocity of 200 miles per second.

This interesting problem has been discussed by Professor Newcomb, whose calculations we shall here follow. In the first place we require to make some estimate of the dimensions of the sidereal system, in order to see whether it seems likely that this star can ever be recalled. The number of stars may be taken at one hundred millions, which is probably double as many as the number we can see with our best telescopes. The masses of the stars may be taken as on the average five times as great as the mass of the sun. The distribution of the stars is suggested by the constitution of the Milky Way. One hundred million stars are presumed to be disposed in a flat circular layer of such dimensions that a ray of light would require thirty thousand years to traverse one diameter. Assuming the ordinary law of gravitation, it is now easy to compute the efficiency of such an arrangement in attempting to recall a moving star.

The whole question turns on a certain critical velocity of twenty-five miles a second. If a star darted through the system we have just been considering with a velocity less than twenty-five miles a second, then, after that star had moved for a certain distance, the attractive power of the system would gradually bend the path of the star round, and force the star to return to the system. If, therefore, the velocities of the stars were under no circumstances more than twenty-five miles a second, then, supposing the system to have the character we have described, that system might be always the same. The stars might be in incessant motion, but they must always remain in the vicinity of our present system, and our whole sidereal system might be an isolated object in space, just as our solar system is an isolated object in the extent of the sidereal system.

We have, however, seen that for one star at all events the velocity is no less than 200 miles a second. If this star dash through the system, then the attractions of all the bodies in the system will unite in one grand effort to recall the wanderer. This attraction must, to some extent, be acknowledged; the speed of the wanderer must gradually diminish as he recedes into space; but that speed will never be lessened sufficiently to bring the star back again. As the star retreats further and further, the potency of the attraction will decrease; but, owing to the velocity of the star being over twenty-five miles a second, the attraction can never overcome the velocity; so that the star seems destined to escape.

This calculation is of course founded on our assumption as to the total mass of the stars and other bodies which form our sidereal system. That estimate was founded on a liberal, indeed a very liberal interpretation of the evidence which our telescopes have afforded. But it must probably fall short of the truth on account of the myriads of dark stars. There may be more than a hundred million stars in our system; the average weight may be more than five times the weight of our sun. But unless the assumption we have made is enormously short of the truth, our inference cannot be challenged. If the stars are sixty-four times as numerous, or if the whole mass of the system be sixty-four times as great as we have supposed, then the critical velocity would be 200 miles a second instead of twenty-five miles a second. Our estimate of the system would therefore have to be enlarged sixty-four fold, if the attraction of that system is to be adequate to recall 1830 Groombridge. It should also be recollected that our assumption of the velocity of the star is very

moderate, so that it is not at all unlikely that a system at least 100 times as massive as the system we have supposed would be required if this star was to be recalled.

The result of this inquiry is to be stated as an alternative: either our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than a liberal interpretation of observations would seem to warrant. If we adopt the first alternative, then we see that 1830 Groombridge, having travelled from an indefinitely great distance on one side of the heavens, is now passing through our system for the first and the only time. After leaving our neighbourhood it will retreat again into the depths of space, to a distance which, for anything we can tell, may be practically regarded as infinite. Although we have only used this one star as an illustration, yet it is not to be supposed that the peculiarities which it presents are absolutely unique. It seems more likely that there may be many other stars which are at present passing through our system. In fact, considering that most or all of the stars are actually in motion, it can be shown that in the course of ages, the whole face of the heavens is gradually changing. We are thus led to the conclusion that our system may not be an absolutely isolated group of bodies in the abyss of space, but that we are visited by other bodies coming from the remotest regions of space.

The whole range of astronomy presents no speculations which have attracted more attention than the celebrated nebular hypotheses of Herschel and of Laplace. We shall first enunciate these speculations, and then we shall attempt to indicate how far they seem to be warranted by the actual state of scientific knowledge. In one of his

most memorable papers, Sir W. Herschel presents us with a summary of his observations on the nebulæ arranged in such a manner as to suggest his theory of the gradual transmutation of nebulæ into stars. He first shows us that there are regions in the heavens where a faint diffused nebulosity is all that can be detected by the telescope. There are other nebulæ in which a nucleus can be just discerned; others again in which the nucleus is easily seen; and still others where the nucleus is a brilliant star-like point. The transition from an object of this kind to a nebulous star is very natural, while the nebulous stars pass into the ordinary stars by a few graduated stages. It is thus possible to enumerate a series of objects, beginning at one end with the most diffused nebulosity, and ending at the other with an ordinary fixed star or group of stars. Each object in the series differs but slightly from the object just before it and just after it.

It seemed to Herschel that he was thus able to view the actual changes by which masses of phosphorescent or glowing vapour became actually condensed down into stars. The condensation of a nebula could be followed in the same manner as we can study the growth of the trees in a forest by comparing the trees of various ages which the forest contains at the same time. In attempting to pronounce upon the positive evidence available in the discussion of Herschel's theory, we encounter a well-known difficulty. To establish this theory, it would be necessary to watch the actual condensation of one single nebula from the primitive gaseous condition down to the stellar points. It may easily be conceived that such a process would require a vast lapse of time, perhaps enormously greater than the period between the invention of the telescope and the pre-

sent moment. It may at all events be confidently asserted that the condensation of a nebula into a star is a process which has never been witnessed. Whether any stages in that process can be said to have been witnessed is a different matter, on which it is not easy to speak with precision. Drawings of the same nebula made at different dates often exhibit great discrepancies. In comparing these drawings, it must be remembered that a nebula is an object usually devoid of distinct outline, and varying greatly in appearance with different telescopic apertures. Take, for instance, the very splendid nebula in Orion, which is one of the most glorious objects that can be seen in a telescope. There can be no doubt that the drawings made at different times do exhibit most marked differences. Indeed, the differences are sometimes so great that it is hard to believe that the same object is depicted. It is well to look also at drawings made of the same object at the same time, but by different observers and with different telescopes. Where we find contemporary drawings at variance—and they are often widely at variance—it seems hard to draw any conclusion from drawings as to the presence or the absence of change in the shape of the nebula.

There are, however, good grounds for believing that nebulæ really do undergo some changes, at least as regards brightness; but whether these changes are such as Herschel's theory would seem to require is quite another question. Perhaps the best authenticated instance is that of the variable nebula in the constellation of Taurus, discovered by Mr. Hind in 1852. At the time of its discovery this object was a small nebula about one minute in diameter, with a central condensation of light. D'Arrest,

the distinguished astronomer of Copenhagen, found in 1861 that this nebula had vanished. On the 29th of December, 1861, the nebula was again seen in the powerful refractor at Pulkova, but on December 12, 1863, Mr. Hind failed to detect it with the telescope by which it had been originally discovered. This instrument had, however, only half the aperture of the Pulkova telescope. In 1868, O. Struve, observing at Pulkova, detected another nebulous spot in the vicinity of the place of the missing object, but this also has now vanished. Struve does not, however, consider that the nebula of 1868 is distinct from Hind's nebula, but he says—

"What I see is certainly the variable nebula itself, only in altered brightness and spread over a larger space. Some traces of nebulosity are still to be seen exactly on the spot where Hind and D'Arrest placed the variable nebula. It is a remarkable circumstance that this nebula is in the vicinity of a variable star, which changes somewhat irregularly from the ninth to the twelfth magnitude. At the time of the discovery in 1861, both the star and the nebula were brighter than they have since become."

This is the best authenticated history of observed change in any nebula. It must be admitted that the changes are such as would not be expected if Herschel's theory were universally true.

Another remarkable occurrence in modern astronomy may be cited as having some bearing on the question as to the actual evidence for or against Herschel's theory. On November 24, 1876, Dr. Schmidt noticed a new star of the third magnitude in the constellation Cygnus. The discoverer was confident that no corresponding object existed on the evening of the 20th of November. The

brilliancy of the new star gradually declined until on the 13th of December Mr. Hind found it of the sixth magnitude. The spectrum of this star was carefully studied by many observers, and it exhibited several bright lines, which indicated that the star differed from other stars by the possession of vast masses of glowing gaseous material. The star was observed by the late Dr. Copeland at the Earl of Crawford's observatory on September 2, 1877. It was then below the tenth magnitude, and of a decidedly bluish tint. Viewed through the spectroscope, its light was almost completely monochromatic, and appeared to be indistinguishable from that which is often found to come from nebulæ. Dr. Copeland thus concludes:—

"Bearing in mind the history of this star from the time of its discovery by Schmidt, it would seem certain that we have an instance before us in which a star has changed into a planetary nebula of small angular diameter. At least it may be safely affirmed that no astronomer discovering the object in its present state would, after viewing it through a prism, hesitate to pronounce as to its present nebulous character."

It should, however, be added that Professor Pickering has since found slight traces of a continuous spectrum, but the object has now become so extremely faint that such observations are very difficult. This remarkable history might be adduced if we wished to procure evidence of the conversion of stars into nebulæ, but for the nebular theory we require evidence of the conversion of nebulæ into stars.

Care must be taken not to exaggerate the inferences to be drawn from the two instances I have quoted—viz., the variable nebula in Taurus and the new star in Cygnus. I

think it more likely that both of these are to be regarded as exceptional phenomena. It is certainly true that they are perhaps the most remarkable instances in which changes in nebulæ have actually been witnessed; but the probability is that the only reason why they have been witnessed is because they were very exceptional. Those who have observed the nebulæ for many years are well assured of the general permanence of their appearance. The nebulæ we have referred to are chosen out of thousands. The ordinary nebulæ appear just as constant as the ordinary bright stars. Every one expects to see Vega in the constellation Lyra; and with equal confidence every astronomer counts on seeing the celebrated annular nebula when he directs his telescope to the same constellation. This permanence is very probably merely due to the stupendous distances at which these objects are placed. Only gigantic changes could be detected, and for these gigantic periods of time would be required. We are bound to believe that heated bodies radiate their heat; and if so they must contract. This general law, which pervades all nature, so far as we know it, seems to be the real basis—indeed, the only basis—on which the nebular theory of Herschel can be maintained. Up to the present, it must be admitted that this theory has received no direct telescopic confirmation.

The nebular theory by which Laplace sought to account for the origin of the solar system seems, from the nature of the case, to be almost incapable of receiving any direct testimony. We shall here enunciate the theory in the language of Professor Newcomb:—

"The remarkable uniformity among the directions of the revolutions of the planets being something which

could not have been the result of chance, Laplace sought to investigate its probable cause. This cause, he thought, could be nothing else than the atmosphere of the sun, which once extended so far out as to fill all the space now occupied by the planets. He conceives the immense vaporous mass forming the sun and his atmosphere to have had a slow rotation on its axis. The mass, being intensely hot, would slowly cool off, and as it did so, would contract towards the centre. As it contracted its velocity would, in obedience to one of the fundamental laws of mechanics, constantly increase, so that a time would arrive when, at the outer boundary of the mass, the centrifugal force due to the rotation would counterbalance the attractive force of the central mass. Then those outer portions would be left behind as a revolving ring, while the next inner portions would continue to contract, until at their boundary the centrifugal and attractive forces would be again balanced, when a second ring would be left behind; and so on. Thus, instead of a continuous atmosphere, the sun would be surrounded by a series of concentric revolving rings of vapour.

"Now, how would these rings of vapour behave? As they cooled off, their denser materials would condense first, and thus the ring would be composed of a mixed mass, partly solid and partly vaporous, the quantity of solid matter constantly increasing and that of vapour diminishing. If the ring were perfectly uniform this condensing process would take place equally all around it, and the ring would thus be broken up into a group of small planets like that which we see between Mars and Jupiter. But we should expect that, in general, some portions of the ring would be much denser than others,

and the denser portion would gradually attract the rarer portions around it, until instead of a ring we should have a single mass, composed of a nearly solid centre, surrounded by an immense atmosphere of fiery vapour. This condensation of the ring of vapour around a single point would have produced no change in the amount of rotary motion originally existing in the ring; the planet surrounded by its fiery atmosphere would therefore be in rotation, and would be, in miniature, a reproduction of the case of the sun surrounded by his atmosphere with which we set out. In the same way that the solar atmosphere formed itself first into rings, and then these rings condensed into planets, so, if the planetary atmosphere were sufficiently extensive, they would form themselves into rings, and these rings would condense into satellites. In the case of Saturn, however, one of the rings was so perfectly uniform that there could be no denser portion to draw the rest of the ring around it, and thus we have the well-known rings of Saturn."

It will thus be seen that one of the principal features in the solar system for which the nebular theory has been invoked is the fact that the planets all revolve round the sun in the same direction. It will therefore be natural to take up first the discussion of this subject, and to inquire how far the common motion of the planets can be claimed in support of Laplace's nebular theory. The value of this argument is very materially influenced by another consideration of a somewhat peculiar character. If it were quite immaterial to the welfare of the planetary system whether all the planets moved the same way, or whether some moved one way and some another, then the nebular hypothesis would be entitled to all the support which

could be derived from the circumstances of the case. Take, for instance, the eight principal planets—Mercury, Venus, the Earth, Mars, Jupiter, Saturn, Uranus, Neptune. All these planets move in the same way around the sun. The chances against such an occurrence are 127 to 1. The probability that the system of eight planets has been guided to move in the same direction by some cause may be taken to be 127 to 1. If we include the two hundred minor planets the probability would be enormously enhanced. The nebular theory seems a reasonable explanation of how this uniformity of movements could arise, and therefore the advocates of the nebular theory may seem entitled to claim all this high degree of probability in their favour. There is, however, quite a different point of view from which the question may be regarded. There are reasons which imperatively demand that the planets (at all events the large planets) shall revolve in uniform directions, which lie quite outside the view taken in the nebular theory. If the big planets did not all revolve in the same direction, the system would have perished long ago, and we should not now be here to discuss the nebular or any other hypothesis.

It is well known that in consequence of the gravitation which pervades the solar system, each of the planets has its movements mainly subordinated to the attraction of the sun. But each of the planets attracts every other planet. In consequence of these attractions, the orbits of the planets are to some extent affected. The mutual actions of the planets present many problems of the highest interest, and, it should be added, of the greatest difficulty. Many of these difficulties have been overcome. It is the great glory of the French mathematicians to

have invented the methods by which the nature of the solar system could be studied. The results at which they arrived are not a little remarkable. They have computed how much the planets act and react upon each other, and they have shown that in consequence of these actions the orbit of each planet gradually changes its shape and its position. But the crowning feature of these discoveries is the demonstration that these changes in the orbits of the planets are all periodic. The orbits may fluctuate, but those fluctuations are confined within very narrow limits. In the course of ages the system gradually becomes deformed, but it will gradually return again to its original position, and again depart therefrom. These changes are comparatively so small that our system may be regarded as substantially the same even when its fluctuations have attained their greatest amplitude. These splendid discoveries are founded upon the actual circumstances of the system, as we see that system to be constituted.

Take, for instance, the eccentricities of the orbits of the planets around the sun. Those eccentricities can never change much; they are now small quantities, and small quantities those eccentricities must for ever remain. The proof of this remarkable theorem partly depends upon the fact that the planets are all revolving around the sun in the same direction. If one of the planets we have named were revolving in an opposite direction to the rest, the mathematical theory would break down. We should have no guarantee that the eccentricities would for ever remain small, as they are at present. In a similar manner, the planets all move in orbits whose planes are inclined to each other at very small angles. The positions of

those planes fluctuate, but these fluctuations are confined within very narrow limits. The proof of this theorem, like the proof of the corresponding theorem about the eccentricities, depends upon the actual conditions of the planetary system as we find it. If one of the planets were to be stopped, turned round, and started off again in the opposite direction, our guarantee for the preservation of the planes would be gone. It therefore follows that if the system is to be permanently maintained, all the planets must revolve in the same direction.

In this connection it is impossible not to notice the peculiar circumstances presented by the comets. By a sort of convention the planets have adopted, or, at all events, they possess, movements which fulfil the conditions necessary if the planets are to live and let live; but the comets do not obey any of the conditions which are imposed by the planetary convention. The orbits of the comets are not nearly circles. They are sometimes ellipses with a very high degree of eccentricity; they are often so very eccentric that we are unable to distinguish the parts of their orbits which we see from actual parabolas. Nor do the directions in which the comets move exhibit any uniformity; some move round the sun in one direction, some move in the opposite direction. Even the planes which contain the orbits of the comets are totally different from each other. Instead of being inclined at only a very few degrees to their mean position, the planes of the comets hardly follow any common law; they are inclined at all sorts of directions. In no respect do the comets obey those principles which are necessary to prevent constitutional disorder in the planetary system.

The consequences of this are obvious, and unfortunate in

the highest degree—for the comets. A comet possesses no security for the undisturbed enjoyment of its orbit. Not to mention the risk of actual collision with the planets, there are other ways in which the path of a comet may experience enormously great changes by the disturbances which the planets are capable of producing. How is it that the system has been able to tolerate the vagaries of comets for so many ages? Solely because the comets, though capable of suffering from perturbations, are practically incapable of producing any perturbations on the planets. The efficiency of a body in producing perturbations depends upon the mass of the body. Now all we have hitherto seen with regard to comets tends to show that the masses of comets are extremely small. Attempts have been made to measure them, but have always failed, because the scales in which we have attempted to weigh them have been too coarse to weigh anything of the almost spiritual texture of a comet. It is unnecessary to go as far as some have done, and to say that the weight of a large comet may be only a few pounds or a few ounces. It might be more reasonable to suppose that the weight of a large comet was thousands of tons, though even thousands of tons would be far too small a weight to admit of being measured by the very coarse balance which is at our disposal.

The enduring stability of the planetary system is thus seen to be compatible with the existence of comets solely because comets fulfil the condition of being almost imponderable in comparison with the mighty masses of the planetary system. The very existence of our planetary system is a proof of the doctrine that the masses of the comets are but small. Indeed, to those who will duly

weigh the matter, it will probably appear that this negative evidence as to the mass of the comets is more satisfactory than the results of any of the more direct attempts to place the comets in the weighing scales. If we restate the circumstances of the solar system, and if we include the comets in our view, it will appear how seriously the existence of the comets affects the validity of the argument in favour of the nebular hypothesis which is derived from the uniformity in the directions of the planetary movements. If we include the whole host of minor planets, we have for the population of the solar system something under three hundred planets, and an enormous multitude of comets. It will probably not be an over-estimate if we suppose that the comets are ten times as numerous as the planets. The case, then, stands thus:

The solar system consists of some thousands of different bodies; these bodies move in orbits of the most varied degrees of eccentricity; they have no common direction; their planes are situated in all conceivable positions, save only that each of these planes must pass through the sun. Stated in this way, the present condition of the solar system is surely no argument for the nebular theory. It might rather be said that it is inconceivable on the nebular theory how a system of this form could be constructed at all. Nine-tenths of the bodies in the solar system do not exhibit movements which would suggest that they were produced from a nebula: the remaining tenth do no doubt exhibit movements which seem to admit of explanation by the nebular theory: but, had that tenth not obeyed the group of laws referred to, they would not now be there to tell the tale. The planetary system now lives because it was an organism fitted for survival.

It is often alleged that the comets are not indigenous to the solar system. It has been supposed that the comets have been imported from other systems. It has also been urged with considerable probability that perhaps many comets may have had their origin in our sun, and have been actually ejected therefrom. I do not now attempt to enter into the discussion of these views, which are at present problematical; let me pass from this part of the subject, with the remark that until the nature and origin of comets is better understood, it will be impossible to appraise with accuracy the value of the argument for the nebular hypothesis which has been based on the uniformity of the directions in which the planets revolve around the sun.

There are, however, other circumstances in the solar system which admit of explanation by the nebular theory. It is a remarkable fact that the Earth, Mars, Jupiter, and Saturn are all known to rotate upon their axes in the same direction as their revolutions around the sun. The nebular theory offers an explanation of this circumstance. It does not appear that this common rotation of the planets is absolutely necessary for the stability of the system. Should it further be proved that there is no other agency at work which would force the planets to rotate in the same direction, then it must be admitted that the nebular theory receives very substantial support.

There is another way in which we can examine the evidence on behalf of the nebular hypothesis. There are certain actions going on at present in the solar system; and by reasoning backwards from these present actions we are led to believe that in extremely early times the condition of things may have resembled that which is supposed

by the nebular hypothesis. Let us begin with the consideration of our sun, which is, as we know, daily radiating off light and heat into space. This heat is poured off in all directions; a small portion of it is intercepted by the earth, but this portion is less than one two-thousand-millionth part of the whole; the planets also, no doubt, each intercept a small portion of the solar radiation; but the great mass of radiated heat from the sun entirely escapes. This heat is supposed not to be restored to the sun. The sun certainly must receive some heat by the radiation from the stars; but this is quite infinitesimal in comparison with its own stupendous radiation. We therefore conclude that the sun's heat is being squandered with prodigal liberality. We also know that the store of heat which the sun can possess, though no doubt enormously great, is still limited in amount. It is, indeed, a question of very great interest to decide what are the probable sources by which the sun is able to maintain its present rate of expenditure. It must have some source of heat in addition to that which it would possess in virtue of its temperature as an incandescent body. If we suppose the sun to be a vast incandescent body, formed of materials which possess the same specific heat as the materials of which our earth is composed, the sun would then cool at the rate of from 5° to 10° per annum. At this rate the sun could not have lasted for more than a few thousand years before it cooled down. We are therefore compelled to inquire whether the sun may not have some other source of heat to supply its radiation beyond that which arises merely from the temperature.

Of the various sources which have been suggested, it will here only be necessary to mention two. It has been

supposed that the heat of the sun may be recruited by the incessant falling of meteoric matter upon the sun's surface. If that matter had been drawn only by the sun's attraction from the remote depths of space, it would fall upon the sun with an enormously great velocity, amounting to about 300 miles a second. It follows from the principle of the equivalence between heat and mechanical energy that a body entering the sun with this velocity would contribute to the sun a considerable quantity of heat. It is known that small meteoroids abound in the solar system; they are constantly seen in the form of shooting stars when they dash into our atmosphere, and it can hardly be doubted that myriads of such bodies must fall into the sun. It does not, however, seem likely that enough matter of this kind can enter the sun to account for its mighty radiation of heat. It can be shown that the quantity of matter necessary for this purpose is so large that a mass equal in the aggregate to the mass of the earth would have to fall into the sun every century if the radiation of the sun were to be defrayed from this source. That so large a stream of matter should be perennially drawn into the sun is, to say the least, highly improbable.

But it is quite possible to account for the radiation of the sun on strictly scientific principles, even if we discard entirely the contributions due to meteoric matter. As the sun parts with its heat it must contract, in virtue of the general law that all bodies contract when cooling; but in the act of contraction an amount of heat is produced. By this the process of cooling is greatly retarded. It can, indeed, be shown that, if the sun contracts so that his diameter decreases one mile every twenty-five years, the amount of heat necessary to supply his radiation would

be amply accounted for. At this rate many thousands of years must elapse before the diminution in the sun's diameter would be large enough to be appreciable by our measurements.

Looking back into the remote ages, we thus see that the sun was larger and larger the further back we project our view. If we go sufficiently far back, we seem to come to a time when the sun, in a more or less completely gaseous state, filled up the surrounding space out to the orbit of Mercury, or, earlier still, out to the orbit of the remotest planet. If we admit that the present laws of nature have been acting during the past ages to which we refer, then it does not seem possible to escape the conclusion that the sun was once a nebulous mass of gas such as the nebular theory of Laplace would require.

It will also throw some light upon this retrospective argument for the nebular theory if we briefly consider the probable history of the earth. It is known that the interior of the earth is hotter than the exterior. It has been suggested that this interior heat may arise from certain chemical actions which are at present going on. If this were universally the case, the argument now to be brought forward could not be entertained. I believe, however, most physicists will agree in thinking that the interior heat of the earth is an indication that it is cooling down from some former condition in which it was hotter than it is at present. The surface has cooled already, and the interior is cooling as quickly as the badly conducting materials of the crust will permit. We are thus led to think of the earth as having been hotter in past time than at present. The further we look back the greater must the earth's heat have been. We cannot stop

till the earth was once red-hot or white-hot, till it was molten or a mass of fiery vapour. I have endeavoured to set forth a popular account of the nebular theory in a volume entitled "The Earth's Beginning."

The verdict of science on the whole subject cannot be expressed better than in the words of Newcomb :—

"At the present time we can only say that the nebular hypothesis is indicated by the general tendencies of the laws of nature; that it has not been proved to be inconsistent with any fact; that it is almost a necessary consequence of the only theory by which we can account for the origin and conservation of the sun's heat; but that it rests on the assumption that this conservation is to be explained by the laws of nature as we now see them in operation. Should any one be sceptical as to the sufficiency of these laws to account for the present state of things, science can furnish no evidence strong enough to overthrow his doubts until the sun shall be found growing smaller by actual measurement, or the nebulæ be actually seen to condense into stars and systems."

CHAPTER X.

IS THE UNIVERSE INFINITE?

BISHOP BUTLER has well remarked that "probability is the guide of life," and, assuredly, if it be our guide in all practical concerns, in a still more significant sense may it be claimed as the source of the greater part of human knowledge. Indeed, in a rapid survey of the field of astronomy we are tempted to affirm, not merely that the theory of probability is of the utmost service to us, but that it is almost our sole method of discovering the truth. This will not seem a paradox to any one who will reflect that there is hardly an astronomical doctrine, even of the most elementary kind, of which it might not be said that our belief in it depends simply on the fact that its truth is, in a high degree, more probable than its falsehood. To those who are accustomed to apply the doctrine of probabilities habitually and, indeed, almost unconsciously, it affords the readiest touchstone by which many fallacious scientific notions can be dissipated.

Let me give an illustration of what I mean. In the first book about astronomy which I read in my boyhood there was a glowing description of an investigation

which at one time seemed to have attracted a great deal of attention. I allude to the discovery, or the alleged discovery I should rather say, of a certain "central sun," about which it was believed or stated that all the bodies in the universe revolved. This marvellous centre was becomingly located in the Pleiades—indeed, if I remember aright, it was actually identified with the star Alcyone. The doctrine was certainly a splendid and captivating one, but it was too good to be true. No one ever hears anything about the central sun hypothesis nowadays, and that, perhaps, for the simple reason that it stood condemned on the face of it by the theory of probabilities. It is wholly unnecessary at this time of day to attempt to appraise the value of the observations by which an astronomer, justly esteemed for other labours, demonstrated, or thought he had demonstrated, the existence of a "central sun." Even if the apparent movements of certain stars offered quite unequivocal testimony (which, indeed, was by no means the case) to show that they were revolving around Alcyone, still it is obvious, on a little consideration, that even this famous star could not be regarded as the centre of the whole universe without doing unwarrantable violence to all notions of probability. For just look at the facts in their due proportion.

Alcyone, no doubt, is a star of magnificent dimensions. It may be a hundred or a thousand times more massive and more brilliant than our sun. Alcyone is so remote from the earth that the light which now arrives at our eyes, even though it speeds on its way at the rate of 186,000 miles a second, has not improbably taken a century, or more than a century, to reach us. The

Pleiades form a cluster of bright stars almost unique in their interest; and these circumstances might certainly render the notion that there lay the centre of the universe highly attractive to the imagination and perhaps even quite plausible. But the theory of probabilities at once upsets the whole doctrine when the facts are viewed in their proper light.

No doubt the theory of probabilities has nothing to say against Alcyone in comparison with any other star visible in the heavens, but what it does say is that it would be utterly preposterous to imagine that any one of the stars in the visible firmament could be the central sun around which all the bodies in the universe revolved. For summon up to your imagination the most distant star that can be seen with the unaided eye. Then think of the minutest star that our most potent telescope can disclose. Think of the tiniest stellar point of light which could possibly be depicted on the most sensitive photographic plate after hours of exposure to the heavens. Think, indeed, of the very remotest star which, by any conceivable device, can be rendered perceptible to our consciousness. Doubtless that star is thousands of billions of miles from the earth; doubtless the light from it requires thousands of years, and some astronomers have said millions of years, to span the abyss which intervenes between our globe and those distant regions. But, nevertheless, there is a certain number of miles, even though we know it not, at which the remotest stars known to us must lie. I do not speak of the most distant star which the universe may possibly contain; I only refer to the most distant star that we can possibly bring within our ken.

Imagine a great sphere to be described with its

centre at our earth, and with a radius extending all the way from the earth to this last star knowable by man. Every star that we can see, every star whose existence becomes disclosed to us on our photographs, lies inside this sphere; as to the orbs which may lie outside that sphere we can know nothing by direct observation. The imagination doubtless suggests with irresistible emphasis, that this outer region is also occupied by stars and nebulæ, suns and worlds, in the same manner as the interior of that mighty sphere whose contents are more or less accessible to our scrutiny. It would do utter violence to our notions of the law of continuity to assume that all the existent matter in the universe happened to lie inside this sphere; we need only mention such a supposition to dismiss it as wholly indefensible. I do not now make any attempt to express the number of miles in the diameter of the sphere which limits the extent of space known directly to man. What that number may be is quite immaterial for our present purpose. But the point that I specially want to bring out is that the volume occupied by this stupendous globe, which includes within it all possible visible material, must be but a speck when compared with the space which contains it. Think of the water in the Atlantic Ocean, and think of the water in a single drop. As the drop is to the Atlantic Ocean so is the sphere which we have been trying to conceive to the boundless extent of space. As far as we know it would seem that there could be quite as many of such spheres in space as there are drops of water in the Atlantic Ocean. And, in all probability, these other spheres throughout space are tenanted by stars, systems, and galaxies just as grand in themselves, just as imposing in their colloca-

tions, and just as overwhelming in their myriads as are those which lie within that one particular sphere of which alone we know anything with certainty.

Provided with this conception, we see at once that the doctrine of a visible central sun is an absurdity. As to whether there may be some central sun somewhere or other I can express no opinion, save that I do not see any reason whatever to think that such a body should exist. But we may feel practically certain, according to all rational grounds of probability, that even if there were a central sun in the universe it would not lie within our ken. Suppose that in the wide extent of the Atlantic Ocean there was one individual diatom of a specially interesting character; I do not mean one speciés with its myriad individuals, but one solitary specimen of a particular microscopic organism, which happened to flourish somewhere or other in the North or South Atlantic Ocean at some depth or other from the surface. Supposing that absolutely nothing further was known as to the whereabouts of this individual object; it might, for anything we could tell, lie beneath a mighty ice-floe in the Arctic regions; it might be miles deep in the Caribbean Sea; it might be basking on the surface in the Equatorial calms; it might be tossed in the surf on the shores of St. Helena; it might be floating at the mouth of the Amazons; it might be off the Cape of Good Hope, or amid the Antarctic icebergs. Would any reasonable man who desired to obtain that unique and extraordinary specimen for his collection imagine that if he went down to the coast of Cornwall and lifted a single drop from the Atlantic he would have such inconceivably good fortune as to find in it this rare diatom of which but a single individual existed through-

out the millions of cubic miles of water which compose that mighty ocean? Of course, the mere statement of such a case is sufficient to show its absurdity. But the improbability that the ardent naturalist would secure the prize in the way I have described is not one whit greater than the improbability that, even if there were a central sun, it should lie within the domain of our scrutiny.

There is another line of reasoning by which the theory of probability will often give us invaluable information, which is not this time merely of a negative kind. There are many instances which might be taken of the principles now to be employed. I shall, however, adopt that particular one which presents, perhaps, the greatest interest to astronomers. The question often arises as to whether two objects which appear to us to lie near each other on the surface of the heavens are really neighbours in space, or whether their contiguity is only apparent. It often happens, for instance, that two stars appear very close together through the telescope, and we desire to know whether the two bodies are indeed allied by any bond of physical association, or whether the appearance may not be a mere accidental coincidence. The latter would be the case if the line joining the two stars happened to be so nearly directed towards the earth that, though in reality one of the stars is so much more remote than the other, yet that from our point of view the two happen to be projected on the same part of the sky. We are generally at fault in determining this question by direct observation, because it is usually impossible to find the actual distances by which the earth is separated from the objects, and, therefore, we are deprived of any direct assurance that those distances are so far equal as to enable

us to assert that the apparent contiguity is indeed a real contiguity. Here the theory of probabilities will come to our aid and supply reliable information of the most convincing character.

The illustration I shall take is one connected with a famous object. The Great Nebula in Orion is known to be the most glorious body of its class that the heavens display. Seen through a powerful telescope, like that of Lord Rosse at Parsonstown, the appearance of this grand "light-stain" is of indescribable glory to one whose previous acquaintance with practical astronomy enables him to inform the picture before him with the knowledge necessary for its comprehension. It is a vast volume of bluish gaseous material with hues of infinite softness and delicacy. Here it presents luminous tracts which glow with an exquisite blue light; there it graduates off until it is impossible to say where the nebula ceases and the black sky begins. But from our present point of view I am only thinking of the nebula as the nimbus of glory which surrounds the marvellous multiple star known to astronomers as Theta Orionis.

This complex sidereal system consists of four bright stars quite close together, with at least two smaller ones which evidently belong to the same scheme. The whole sextuple group makes a spectacle unique in the heavens. Wherever Theta Orionis happened to be in the sky it must necessarily be known as the most elaborately composed of all multiple stars. But, as a matter of fact, we find the wonderful star apparently occupying the most imposing site in the Great Nebula, so that the latter serves as a splendid setting to the complex star. The appearance presented would, of course, be explained if it should

happen that the wondrous multiple did actually lie inside the nebula wherein it was seen gleaming. But it is, no doubt, conceivable that the effect actually witnessed might be accounted for if it should happen that the multiple star were billions of miles in the foreground, only so placed that from our point of view we beheld it projected with the brightest part of the nebula as a background. Such, too, is the translucency of nebulous material that it is at least a conceivable hypothesis that the nebula might be the object which lay in the foreground and that the star occupied a position billions of miles in the rear, but that from where we were situated our line of sight towards the star conducted our vision directly through the centre of the nebula.

Fig. 31.—The Multiple Star (Theta Orionis) in the great nebula of Orion.

We have really no means of certainly knowing which of these notions is the correct one. At least, I should say, direct observation cannot be held to have shown conclusively that one of these doctrines is true and that the other two are false. It could only have done so when we had measured the distances of both the nebula and the multiple star from the earth. As a matter of fact we have not measured the distance of either the one or the other. This is eminently a case in which the theory of probabilities can be suitably applied, and the result to which it leads is of no uncertain kind. It demonstrates, by a line of reasoning the cogency of which cannot be impugned, that the famous stars are not

standing out in front of the nebula, that they are not sunk far behind, but that they do veritably lie at the heart of the nebula itself, the combined object forming one glorious organization. To simplify the application of the argument, let us assume that the visible heavens are constituted, not of hosts of stars and nebulæ, but of one single star and one single nebula. Let us suppose that the nebula occupies an area of about one square degree, that is, about five times the area of the full moon, and let us suppose that from our point of view the star appears to lie within the confines of the nebula. Would it be more reasonable to believe that the presence of the star in that particular locality of the heavens was only an accidental circumstance due to the line of vision from the nebula to the star passing through the eye, or that it was due to the fact that there was some physical connection between the two bodies, in which case, of course, the star would lie within the confines of the nebula, and the contiguity would be real as well as apparent?

Suppose that the star and the nebula were both planted down absolutely at random on the surface of the heavens; then, as the nebula occupies a space of one square degree, and as there are forty thousand square degrees on the surface of the sphere, there are obviously forty thousand chances to one against the star happening to lie within the confines of the nebula, if the connection between the two bodies were merely casual and apparent. For the ordinary purposes of life, when we find that there are forty thousand chances to one against a particular phenomenon occurring, we generally exclude from the realm of practical duty the supposition that the unlikely event will occur. If a sum of

£150 is to be raffled by the sale of enough tickets at a penny a-piece to leave a reasonable profit on the undertaking, the purchaser of a ticket builds but little hope on his chances of success. He knows that the chances against him are about forty thousand to one. We are entitled to say that there must be forty thousand chances to one against the star lying in the nebula, unless it should happen that there was some physical connection between the two. We see, however, that the star does lie in the nebula; therefore, for all practical purposes, we conclude that there must be some physical reason for this coincidence, but we can see no physical reason whatever why the line joining the star and the nebula should pass near the earth if the two objects were totally distinct. We are, therefore, forced to the conclusion that the star must be directly associated with the nebula. There are forty thousand chances to one that this is the case, and, as rational people, we adopt this conclusion as the basis of our belief.

This will illustrate the argument used in the actual case of the Great Nebula in Orion and the multiple star in the same constellation. It is true that there are thousands of stars and thousands of nebulæ, but there is only one star so marvellously complex in its character as Theta Orionis, and there is only one nebula so ample in its magnificence as that in the sword-handle of Orion. But we find the unique multiple star apparently located in the richest part of the unique nebula. If, therefore, we remember that the region of the nebula referred to is perhaps about a square degree in extent, we are entitled to affirm that there must be forty thousand chances to one that Theta Orionis, the star, is veritably immersed in the

glorious nebulosity of Orion. The theory of probabilities allows reasonable beings to draw no other conclusion.

The theory of probabilities is also very instructive in the information which it gives us with reference to the existence of an invisible myriad of bodies through space which can never be discerned by any means at our disposal. It is, of course, well known that the stars, properly so-called, are each of them brilliant suns, intrinsically of majestic proportions, but dwarfed to comparative insignificance by the tremendous distance in space at which they are placed. These bodies are all self-luminous, and it may no doubt happen that there are dark bodies in the vicinity of some of the bright stars to which these stars act as illuminants, just in the same way as the sun dispenses light to the planets. But it is utterly impossible for us to discern objects illuminated in this fashion, for the light which they receive from suns that lie in their neighbourhood would be altogether insufficient to render them visible to us across the vast abyss of space by which they are separated from the earth. There are, no doubt, certain indirect processes of reasoning by which astronomers have learned, with more or less accuracy, something with regard to these dark stars. Thus, for instance, it has been shown that the extraordinary fluctuations in the light of Algol must be attributed to the eclipses of a brilliant star by the interposition at regular intervals of a dark body revolving around it. There are also cases in which it has happened that two dark stars have come so near each other that they may be almost said to have collided, and the sudden transformation of energy of motion into energy of light and heat has been sufficient to announce far and wide through the universe the character of the event which has taken place.

But such instances are few and far between, and we should receive a very erroneous impression as to the population of the celestial regions by bodies devoid of light if we thought that the few whose presence has been occasionally disclosed in some very indirect and casual manner bore anything like a considerable proportion to the total number which actually exist. It is just at this point that the theory of probabilities comes to supplement our knowledge, and the results to which it conducts us are of a most startling description. By this theory we are assured, with a logic which cannot be controverted, that the invisible bodies must be vastly more numerous than the visible stars, so that even the millions of bright stars which we see afford only an utterly inadequate conception of the full extent of the material universe. Remember, I am not now referring to objects beyond our ken merely because they lie so far off. What I mean is that even within the sphere which contains the visible stars that we know, there is such a stupendous quantity of matter of a dark character, that the visible part bears an almost imperceptible proportion to it. It may well be asked how we know that there is this exuberant abundance of invisible matter. Let the theory of probabilities answer the question.

I shall suppose that we have to deal with a lapse of time, which for our present argument may be regarded as indefinitely long. It can be demonstrated that the conditions under which a mass of matter becomes so highly heated as to shine with star-like radiance are wholly exceptional in their character. So far as our present knowledge goes it would seem that the brightness of any sun-like body is to be attributed solely to the transforma-

tion in some fashion of mechanical power into heat. To take our own sun as an example, it is now an assured doctrine that the heat so necessary for our welfare is sustained by the gradual contraction of the solar volume. The energy available for transformation into heat in this process seems sufficient to supply the radiation of the sun, not only for ages such as those which we reckon in the human period, but even throughout a lapse of time so vast as that which geology demands for the formation of the earth's crust. But it is certain that the quantity of possible light and heat to be dispensed by the sun is limited in amount. The sun cannot shine on for ever. A time must assuredly come when the mighty orb at present so brilliant will have no more potency for the radiation of light than is at present possessed by the earth or the moon.

In like manner it can be shown that the materials constituting the sun have not always been luminous. We cannot indeed say with certainty by what influence their brightness was originally kindled. It probably arose from a collision, or an approach to a collision, between two dark masses which happened to come to an encounter with enormous velocities in their progress through space. It is, however, plain that the ages during which the sun has been brilliant form only an incident, so to speak, in the infinite history of that quantity of matter of which the solar system is constituted. Notwithstanding the millions, or thousands of millions, of years for which that matter has existed, it has perhaps only once become so heated, owing to circumstances which we may describe as accidental or casual, as to have acquired the ample light-dispensing power of a sun. It is, however, possible that such periods of light-radiating capacity should have

occurred more than once; they may possibly have occurred several times throughout the ages of time past. Nor is it likely that the last phenomena of this kind have yet arrived.

The sun, after the lapse of countless years, will lose all its heat and pass into a dark mass. In that form it may endure for a period so protracted that the spell during which it has acted as the luminary to our system will appear but a moment in comparison with the dark ages which succeed the solar splendour. But we can conceive that the darkness, which is the doom of our system, need not necessarily be eternal so far as its materials are concerned; it may be that again in the course of its wanderings through space, the tide of chance may at length bring the dark and tremendous globe so near some other orb that another collision should take place with appalling vehemence. The solid materials may again become transformed into a stupendous glowing nebula, and then, in the course of the tedious contraction of this nebula, another protracted period of brilliance will diversify the career of this great body, and may last long enough for the evolution of planets and of whole races of highly organized creatures.

The essential point for our present consideration must not be misunderstood. A little reflection will show that any periods of brilliance must be regarded as exceptional periods in the history of each body. Think, for instance, of all the iron on the surface of the earth. There is the iron in the ore; there are the great stores of pig-iron lying ready for use; there are the vast bridges which span our rivers and straits; there are the thousands of miles of railway lines; there are the

countless wheels and pieces of machinery; there are iron vessels on every ocean, and objects of every size made of iron, from the smallest nails up to hundred-ton guns. There is also at this moment, and every moment, a good deal of hot iron on the earth. While I write, iron is doubtless flowing from blast furnaces in England, Wales, and Scotland; while I write, ingots of white-hot Bessemer steel are being dealt with under the steam-hammer or in the rolling-mills; while I write, horse-shoes are being forged, and, at each moment, in one way or another, pieces of iron of every temperature could be found, from those which are as cold as the iron apparatus used by Sir James Dewar in his experiments in the liquefaction of air, up to the glittering melted steel which is poured from the tilted converter. But it must be admitted that the highly-heated pieces of iron bear a very small proportion indeed to the total mass of iron in the world at any moment. No doubt there are many tons of iron now white-hot, but there are many millions of tons of iron once white-hot, but now no warmer than the air around. At certain phases in its history every piece of iron has to undergo the operation of being raised to incandescence, or even of being transformed into a liquid. But the laws of cooling are such that, as soon as the opportunity is afforded, the iron parts with its redundant heat and returns to a stable condition, in which it is at the temperature of the air.

Suppose that some percipient being, who was viewing this earth from above, could only recognise iron when it was red-hot or white-hot, but that he had every facility for perceiving such iron as happened to be in this condition. With such faculties, he would, no doubt,

be able to discern here and there a stream of molten iron issuing from a blast furnace, or perhaps to witness the operation of the forging of an anchor under the steam-hammer, to watch the rolling of the plates for an armour-clad, or to see the more humble operations of the blacksmith or nail-maker. But he would surely form an entirely erroneous impression as to the quantity of iron on this earth, or as to the extent in which it was employed in the varied purposes of the arts, if he concluded that there was no iron on our globe at all except that which happened at the moment to be in that particular incandescent state in which alone it was visible to him. If he were gifted with reasoning powers he would say, "It is quite true that I can only see the iron while it is red-hot, but I know that for iron to be red-hot on the earth's surface is an exceptional and abnormal condition of a very temporary or intermittent character. No doubt, every piece of iron may have to be red-hot once, or more than once, but the total duration of such phases of incandescence is quite insignificant under ordinary circumstances when compared with the periods in which the iron is cold and invisible. I, therefore, cannot refuse to believe that there must be an amount of iron on the earth which I do not see, but which bears a proportion to that which I do see in the ratio of thousands or millions to one."

Precisely similar is the way in which the astronomer who is properly familiar with the theory of probabilities will approach the study of the stars. He will reflect that each mass of matter must be cold and invisible for by far the greater part of the period of its existence; he will reflect that on rare occasions, separated by intervals

of appalling length, certain exceptional conditions arise by which this dark piece of matter may be so kindled that, for an epoch, long it may be in years but brief indeed when compared with the span of its total existence, the body would glow as a star.

Provided with this conception let us look on the universe with its millions of orbs. These orbs will be found in every state possible to such bodies; but the enormous majority of them must, in accordance with the principles just laid down, be in the dark and invisible state. Out of some millions it may perhaps be concluded that, at any particular moment, a dozen or so might, by accidental circumstances, be in those phases of their several careers in which luminosity is a characteristic. In such cases only will the orbs be visible. The instructed astronomer will, therefore, believe that the non-visible orbs must be hundreds, thousands, or perhaps millions of times more numerous than those which he can see. When we remember that, by our telescopes and on our photographs, we can discern something like one hundred million luminous stars in the sky; when we remember that every one of these is the indication of a wholly exceptional incident in the career of the body from which the light emanates; and when we further believe, as believe we must, that for each one star which we can thus see there must be a stupendous number of invisible masses, then, indeed, we begin to get some notion of the extraordinary multitude in which material orbs are strewn through space. The theory of probabilities declares to us with a certainty, hardly, in my opinion, inferior to that of optical demonstration, that even within the distance which can be penetrated by our telescopes the visible stars cannot form the

hundredth, probably not the thousandth, perhaps not the millionth part of the total quantity of matter.

On the question as to whether space is finite, our observations with the telescope have but little information to give. The question here involved is rather of a metaphysical complexion. The extent of space depends more upon the facts of consciousness than upon those of astronomical observation. It may, perhaps, simplify the discussion of the subject if we first of all consider the question as to whether the quantity of matter in the universe may be presumed to be infinite or not. We can put the question into a perfectly concise form by reflecting that every particle of matter, whether solid, liquid, or gaseous, is composed of molecules. No doubt these molecules are so numerous that even in the air we breathe the capacity of a lady's thimble would contain a multitude of molecules so great that it has to be enumerated by billions. But we are not at present merely concerned with the actual number of molecules that may exist in the atmosphere, even in its whole extent, or in the whole earth, or in the whole sun. Let us try to conceive the number of molecules that are present in all the stars, bright and dark, which exist not only within those regions of space accessible to our telescopes, but elsewhere as well. In short, let us try to conjure up in our imagination the kind of figures which are to express the total number of molecules in the universe. Is that number finite, or is it not?

This is, perhaps, one of the most fundamental questions in nature which could possibly be proposed. Let us consider the consequences which would follow from adopting a negative answer to this question. If we suppose that the number of molecules is indeed infinite, then we are

necessarily forced to admit at once that space must be infinite too; for had space any boundary, then, since the molecules do not admit of being crowded together beyond a certain extent, it would be impossible that they could exist in infinite abundance. Adopting the sound principle that we need not assume more than is necessary to explain the phenomena actually presented by our consciousness, it seems to me to be clear that the number of molecules of matter in the universe must be finite. The row of figures which would express that number, whatever it may be, is the most remarkable descriptive constant which the universe possesses. It matters not for our present argument what may be the range of figures by which this number can be expressed. It may not be too large to be written even on the thumb-nail by the compendious method of notation now in general use.

Let us next see whether we can learn anything as to the extent of space itself. It is apparent that we seem to be in the presence of about equal difficulties whether we attempt to think of space as finite or as infinite; for, imagine that you go up in a straight line into the sky, and on, and on, and on, in thought for millions of miles, it would seem that the journey ought to be endless; for, supposing that you try to conceive a boundary, the imagination will equally suggest that there is something on the other side of that boundary from which you can commence again. It appears almost equally impossible to suppose that the journey could be carried on for ever as to suppose that it could ever be brought to an end. It was, however, long ago shown by Kant that space was rather to be regarded as a form in which the human mind was compelled to regard objects than as a self-existing fact

of external nature. We have no power in our own consciousness to surmount the difficulties of conception to which I have referred. They arise from the conditions of our mental constitution, and reasoning about space will do no more to remove its mysteries than it will suffice to give to the man born blind a notion of the colour scarlet. But mathematicians, while fully aware of the imperfection of their powers of conception as regards the facts of space, are still enabled to frame a perfectly consistent theory according to which the observed phenomena of nature can be presented within a space which is finite in dimensions. They are even able, as it were, to lay their finger upon the exact point in which the subjective difficulty has arisen.

I must here be permitted to refer to a point in connection with the elements of Euclid. The beginner who studies that work commences, of course, by learning the axioms, and reads without any feeling of discontent or querulousness such venerable truths as that "the whole is greater than its part." But, after a number of propositions of this eminently unquestionable but somewhat puerile kind, he is suddenly brought up by the famous twelfth axiom in which Euclid lays down the theory of parallel lines. Here is a statement of a radically different kind from such assertions as that "if equals be added to equals the wholes are equal." In fact, Euclid's notion of parallel lines is so far from being an axiom of the same character as those other propositions that it is quite possible to doubt its truth without doing any violence to our consciousness.

The principle assumed in the twelfth axiom cannot be proved, and it has been well remarked, that it indicates

the supreme genius of Euclid to have expressed this particular axiom in such language as challenges at once the attention and the caution of the student. It may, however, be said that nearly all our difficulties in connection with the conceptions of space take their origin in the ambiguities which arise from the assumption which the twelfth axiom implies. Some modern mathematicians have gone so far as to deny the existence of this axiom altogether as a truth of nature, and it is most important to notice that when free from the embarrassment which the assumption of Euclid involves, a geometry emerges which removes our difficulties. It seems to show that space is finite rather than infinite, so far as we can assign definite meaning to the words, but it would lead me into matters somewhat inconvenient for these pages if I were to pursue the matter with any further detail. I may, however, say that it can be demonstrated that all known facts about space are reconcilable with the supposition that if we follow a straight line through space—using for the word straight the definition which science has shown properly to belong to it—then, after a journey which is not infinite in its length, we shall find ourselves back at the point from which we started. If anyone should think this a difficulty, I would recommend him to try to affix a legitimate definition to the word straight. He will find that the strictly definable attributes of straightness are quite compatible with the fact that a particle moving along a straight line will ultimately be restored to the point from which it departed.

It is quite true that this seems to be a paradox, but it will not be so considered by the geometer. The truth it implies is indeed quite a familiar doctrine in modern

geometry. But what is not so familiar to mathematicians is that the restoration of the travelling particle to the point from which it originally started need not involve a journey of infinite length. If we assume Euclid's twelfth axiom to be true, then no doubt the traveller can only get back to the point from which he started as the result of a journey of infinite length which will have occupied an infinite time. But now suppose that Euclid's twelfth axiom be not true, or suppose that, what comes to the same thing, the three angles of a triangle are not indeed equal to two right angles, then the journey may require neither an infinite lapse of time nor an infinitely great speed before the traveller regains his original position, even though he be moving in a straight line all the time. Space is thus clearly finite; for a particle travelling in a straight line with uniform speed in the same direction is never able to get beyond a certain limited distance from the original position, to which it will every now and then return.

Those who remember their Euclid may be horror-struck at the heresy which suggests any doubt as to the sanctions by which they believe in the equality of the three angles of a triangle to two right angles. Let them know now that this proposition has never been proved, and never can be proved, except by the somewhat illogical process of first assuming what is equivalent to the same thing, as Euclid does in assuming the twelfth axiom. Let it be granted that this proposition is to some very minute extent an untrue one—there is nothing we know of which shows that such a supposition is unwarrantable—no measurements that we can make with our instruments, no observations that we can make with our telescopes, no

reasonings that we can make with our intellect, can ever demonstrate that the three angles of a triangle may not as a matter of fact actually differ from two right angles by some such amount as, let us say, the millionth part of a second. This does no violence to our consciousness, while it provides the needed loophole for escape from the illogicalities and the contradictions into which our attempted conceptions of space otherwise and us.

CHAPTER XI.

HOW LONG CAN THE EARTH SUSTAIN LIFE?

IT seems to be worth while to collect together what may be said on the subject of the duration of life on the globe viewed as a problem in physics, and this is the subject I propose to discuss in the present chapter.

In the first place, it will be desirable to define a little more clearly the exact question which is to engage us, so as to avoid raising collateral inquiries on which it would not be convenient now to enter. Let it be first of all understood that I am not intending to discuss at present the question from its biological point of view, at least not more than to allude to the conceivability that there can be biological reasons for anticipating a termination to man's existence at some time or other. Why, it may be asked, should the human species expect to enjoy perennial existence, seeing that the facts of palæontology show us that multitudes of races of animals have had their little day, and vanished? It would, at least, be necessary for man to see clear grounds for his belief before he could fancy himself entitled to an immunity from the destruction which seems to be the destiny of other species.

Biological agents for the extinction of man have been suggested with plausibility. The influenza bacillus was

lately rampant over the world. Is there any security against some other bacillus quite as ubiquitous, and ten times as fatal, coming to take its abode among us? It may be that the intelligence of man shall be able to cope with the deadly influences that are around him, and that thus the human race may be preserved from the annihilation that seems to await all unintelligent races of animals. The Pasteurs of the future may be able to devise means by which the ravages of the bacilli in the human body can be still further restrained, even if not wholly frustrated. The advent of intelligent beings on the globe has certainly introduced a factor into evolution the full import of which we are not at present able to appreciate.

Speaking broadly, we may assert that every species of animal gradually vanishes, or is transformed into what may be considered a creation of different character. There are, of course, a few apparent exceptions among organizations of a low type. But the instances of such identities are comparatively few, and they are not to be met with among animals of the higher type. Though some of the lower animals to which we have referred may be of more abiding duration than the higher forms, yet it by no means follows that any of the lower types are qualified for indefinitely long existence. It seems much more likely that, when sufficient time has elapsed, they will not be found exceptions to the law that the duration of every species is limited. The palæontological evidence, so far as it goes, must therefore be held to suggest that the present human animal, like every other species, is necessarily doomed to disappear, unless in so far as the presence of intelligence may be able to avert the fate that seems to attend every species in which intelligence is absent.

How far intelligence may be able to accomplish this is a point on which palæontology gives no guidance whatever. Would the plesiosaurus, if he had been gifted with reasoning power, have been able to do such battle for his race that they would have survived those changes and chances which have certainly swept such creatures from existence? Without speculating on such a question, we may, nevertheless, believe that intelligence can sometimes confer on the species which possesses it a degree of pliancy in accommodating itself to altered conditions of the environment superior to that enjoyed by organisms without intellectual power. It may be noted that man has preserved at least one species of animal from the extinction which to all appearance would otherwise have overtaken it. The camel, as a wild animal, is said to be now extinct. Its nearest ally at present living in a state of nature must be sought in the New World. The camel itself and its immediate congeners have apparently been so extirpated as wild animals, that it is to the llamas and alpacas of Peru that we have to look for the nearest wild animals to the ship of the desert, which has from time immemorial been domesticated in the East. It is at least conceivable that what man has been able to do for other races of animals he can also do on behalf of that race to which he himself belongs.

Suppose that the succession of summer and winter, of seedtime and harvest, were to last indefinitely; suppose that the sun were never to be less generous in the dispensing of his benefits than he is at present, it is quite possible that man's intelligence might be able to defeat various enemies which threaten the extinction of his species. It seems useless for us to discuss this question,

for it is perfectly certain that though man might successfully combat some of the agents seeking for his destruction, there is one that it would be wholly beyond his power to subdue. An agent over which he has and can have no control whatever imposes a term to his existence; nor does it seem possible for human intelligence to avert the threatened doom. To point out the necessity for this conclusion is my object in this chapter.

I know that in the present day there are many who seem to think that hardly any boundaries can be assigned to the resources of a reasoning being. I have heard that when King Hudson in the zenith of his fame was asked as to what his railways were to do when all the coal was burned out, he replied that by that time we should have learned how to burn water. Those who are asked the same question now, will often reply that they will use electricity, and doubtless think that they have thus disposed of the question. The fallacy of such answers is obvious. A so-called "water-gas" may no doubt be used for developing heat, but it is not the water which supplies the energy. Trains may be run by electricity, but all that the electricity does is to convey the energy from the point where it is generated to the train which is in motion. Electricity is itself no more a source of power than is the rope with which a horse drags a boat along the canal. There is much more philosophy in the old saying, "Money makes the mare to go," than in the optimistic doctrine we often hear spoken of with regard to the capacity of man for dealing with nature.

The fact is that a very large part of the boasted advance of civilisation is merely the acquisition of an increased capability of squandering. For what are we doing every

day but devising fresh appliances to exhaust with ever greater rapidity the hoard of coal? There is just a certain number of tons of coal lying in the earth, and when these are gone there can be no more forthcoming. There is no manufacture of coal in progress at the present time. The useful mineral was the product of a very singular period in the earth's history, the like of which has not again occurred in any noteworthy degree in the geological ages that have since run their course. Our steam-engines are methods of spending this hoard; and what we often hear lauded as some triumph in human progress is merely the development of some fresh departure in a frightful extravagance. We would justly regard a man as guilty of expending his substance wastefully if he could not perform a journey without a coach-and-six and half-a-dozen out-riders, and yet we insist that the great steamers which take us across the Atlantic shall be run at a speed which requires engines, let us say, of 12,000 horse-power. If the number of passengers on such a vessel be set down as 500, we have for each passenger the united force of 24 horses, night and day, throughout the voyage. I expect our descendants will think that our coal-cellars have been emptied in a very wasteful manner, particularly when they reflect that if we had been content with a speed somewhat less than that at present demanded the necessary consumption of coal would have been reduced in a far greater proportion than the mere alteration of speed would imply.

Of course, no one will contend that the exhaustion of coal means the end of the human race; man lived here for tens of thousands of years before he learned how to use coal. There may be a sort of Chinese-like civilisa-

tion quite compatible with the absence of mineral fuel, at all events in regions where the climate is tolerably mild. We must also remember, as has often been pointed out, that there are vast stores of energy available elsewhere. The radiation from the sun, if it could be suitably garnered up and employed both directly as heat and indirectly as a source of power, would be quite capable of supplying all conceivable wants of humanity for ages. It is also to be noted that we live on the outside of a globe the inside of which is filled with substances that appear, from all we can learn, to have a temperature not less than that of molten iron. If the crust could be pierced sufficiently far, vast indeed is the quantity of heat that might be available. We see the operation of tapping the internal heat going on in nature. Every volcanic outbreak, every spring of hot water, every geyser are but indications of the internal heat of our globe. It may indeed be hard to see how a practical method for drawing on this vast reserve of heat can be devised, but it is at least conceivable that it may be rendered available when the coal and other more accessible sources have become exhausted, or even when their yield has considerably lessened.

The coal of England may last a century or two; the coal in other parts of the globe may supply our cellars for a few centuries more, but the exhaustion of this truly marvellous product is proceeding at an accelerated pace. Doubtless the end of the coal, at least as an article of a mighty commerce, will arrive within a period brief in comparison with the ages of human existence. In the history of humanity from first to last the few centuries through which we are now passing will stand out prominently as the coal-burning period.

It is a noteworthy fact that the possibility of the continued existence of the human race depends fundamentally upon the question of heat. If heat, or what is equivalent to heat, does not last, then man cannot last either. There is no shirking this plain truism. It is therefore necessary to review carefully the possible sources of heat and see how far they can be relied upon to provide a continuous supply.

Of course it is obvious that the available heat generally comes from the sun. It may be used directly, or it may be and often is used indirectly, for nothing can be more certain than that it is sun heat in a modified form which radiates from a coal fire in the drawing-room or from a log fire in the backwoods. As the sun shines on the growing vegetation, the leaves extract the warmth from the sunbeams. The organism wants carbon, and to obtain it decomposes the carbonic acid gas of which a certain proportion is always present in the air. To decompose this gas requires the expenditure of heat or of what is equivalent to heat. But this does not show itself in raising the temperature of the carbon and oxygen after they have been dissociated. Their temperature may be no higher than was that of the carbonic acid from which they have come, but the heat has been expended in the process of forcing the several molecules asunder from the close and intimate union of their combined condition.

As the growing plant must have carbon, it draws that carbon from the atmosphere, and the heat that is required to effect the decomposition of the carbonic acid is obtained from sunbeams. When the carbon thus derived by the plant comes ultimately to be burned it reunites with the oxygen of the air, and in the act of doing so evolves an

amount of heat precisely equivalent to that which was absorbed from the sunbeams. Thus it is that the heat now radiating from our fireplaces has at some time previously been transmitted to the earth from the sun. If it be timber that we are burning, then we are using the sunbeams that have shone on the earth within a few decades. If it be coal, then we are retransforming to heat the solar energy which arrived at the earth millions of years ago.

The question as to the continued existence of man on this globe resolves itself eventually into an investigation as to the permanence of the heat-supply. Doubtless human life requires many other conditions, but of this we may feel assured, that if the heat fail and if nothing else be forthcoming which can be transformed into heat, then most assuredly from this cause alone there is a term to human existence. Before discussing the prospect of the duration of sunbeams we may first consider a few other less important sources of heat. So far as the coal goes, we have already observed that as it is limited in quantity it can offer no perennial supply. Doubtless there is in the earth some quantity of other materials capable of oxidation, or of undergoing other chemical change; in the course of which, and as an incident of such change, heat is evolved. The amount of heat that can possibly arise from such sources is strictly limited. There is in the entire earth just a certain number of units of heat possible from such chemical combinations, but after the combination has been effected there cannot be any more heat from this source.

Then as to the internal heat of the earth due to the incandescent state of its interior. Here there is no doubt

a large store of energy, but still it is of limited quantity, and it is also on the wane. This heat is occasionally copiously liberated by volcanoes, but ordinarily the transit of heat from the interior to the surface and its discharge from thence by radiation is a slow process. It is, however, sufficient for our present purpose to observe that slow though the escape may be, it is incessantly going on. There is only a definite number of units of heat contained in the interior of the earth at this moment, and as they are gradually diminishing, and as there is no source from which the loss can be replenished, there is here no supply of warmth that can be relied on permanently. It must also be mentioned that there exists another store of energy which under certain conditions admits of being transformed into heat. I allude to the energy which the earth possesses in virtue of its rapid rotation on its axis.

In this respect we may liken our globe to a mighty flywheel which contains a certain quantity of energy that must be poured forth as its speed is reduced. It is the action of the tides which enables this form of earth energy to be transformed into heat. The tides check the speed with which the earth rotates. The energy thus lost must in part at least be transformed into heat, which is then again lost by radiation into space. Of course the quantity of energy which the earth possesses by reason of its rotation is of limited amount, and it is steadily being dissipated, just as the internal heat is being lost and just as the potential heat that exists in consequence of unsatisfied chemical attraction is also declining. It seems that whenever the tides shall have so checked the earth that it only rotates at half its present speed, the quantity

of the energy now existing in consequence of the rotation will have been reduced to a fourth of its present value.

Next as to the various forms in which sun-heat is received. We have already referred to the mode in which it is captured by growing plants. There is also another indirect method in which the sun-heat is made to provide energy useful to man. The waterfall which turns the mill-wheel is of course really efficient because the water is running down, and it can only run down because it has first been raised up. This raising is accomplished by sunbeams. They beat down on the wide expanse of the great oceans, and the vapour thence arising soars aloft into the heights of the atmosphere where it forms clouds. It is of course the solar energy that has performed this task of lifting, and as the rain descends it becomes collected into the streams and rivers which on their way to the sea are made to turn the water-wheels. In like manner it is of course the action of the sun which sets in motion great volumes of air to form the winds, so that when we employ windmills to grind our corn we are utilising energy diffused from the sun.

It goes without saying that the welfare of the human race is necessarily connected with the continuance of the sun's beneficent action. We have indeed shown that the few other direct or indirect sources of heat which might conceivably be relied upon are in the very nature of things devoid of the necessary permanence. It becomes therefore of the utmost interest to inquire whether the sun's heat can be calculated on indefinitely. Here is indeed a subject which is literally of the most vital importance so far as organic life is concerned. If the sun shall ever cease to shine, then must it be certain that there is a term

beyond which human existence, or indeed, organic existence of any type whatever, cannot any longer endure on the earth.

We may say once for all that the sun contains just a certain number of units of heat actual or potential, and that he is at the present moment shedding that heat around with the most appalling extravagance. No doubt the heat-hoard of the sun is so tremendous that the consequences of his mighty profusion do not become speedily apparent. They are indeed, it must be admitted, hardly to be discerned within the few brief centuries that the sun has been submitted to human observation. But we have grounds for knowing as a certainty that the sun cannot escape from the destiny that sooner or later overtakes the spendthrift. In his interesting studies of this subject, Professor Langley gives a striking illustration of the rate at which the solar heat is being squandered at this moment.

He remarks that the great coalfields of Pennsylvania contain enough of the precious mineral to supply the wants of the United States for a thousand years. If all that tremendous accumulation of fuel were to be extracted and burned in one vast conflagration, the total quantity of heat that would be produced would no doubt be stupendous, and yet, says this authority, who has taught us so much about the sun, all the heat developed by that terrific coal fire would not be equal to that which the sun pours forth in the thousandth part of each single second. When we reflect that this expenditure of heat has been going on not alone for the centuries during which the earth has been the abode of man, but also for those periods which we cannot estimate, except by saying

that they are doubtless millions of years, during which there has been life on the globe, then indeed we begin to comprehend how vast must have been the capital of heat with which the sun started on its career.

But now for the question, of supreme importance so far as organic life is concerned, as to the possibility of the indefinite duration of the sun as a source of radiant energy. It may indeed be urged that there is no apparent decline in the warmth of the sun and the brilliancy of the light that it diffuses. There is no reason to think from any historical evidence, or indeed from any evidence whatever, that there is the slightest measurable difference between the radiance of the sun that was shed on the inhabitants of ancient Greece and the radiance that still falls on the same classic soil. So far as our knowledge goes, the plants that now grow on the hills and plains of Greece are the same as the plants which grew on the same hills and plains two thousand years ago. It is, of course, true that the significance of the argument is affected by the circumstance that organisms by the influence of natural selection can preserve a continuous adaptation to an environment which is gradually becoming modified. The olive grows in Greece now, and a tree called by the same name grew there a couple of thousand years ago. I do not suppose that anyone is likely to doubt that the ancient olive and the modern olive are at all events so far alike that plants identical in every respect with the olive of ancient times could flourish where the modern olive now abounds. That there have been great climatic vicissitudes in times past is of course clearly shown by the records of the rocks. It is almost certain that astronomical causes have been largely concerned in

the production of these changes, but from among these causes we may exclude the variations in the sun's heat. There does not seem to be the least reason to suppose that any alteration in the rate at which the sun diffuses heat has been a cause of the vicissitudes of climates which the earth has certainly undergone within geological times.

And yet we feel certain that the incessant radiation from the sun must be producing a profound effect on its stores of energy. The only way of reconciling this with the total absence of evidence of the expected changes is to be found in the supposition that such is the mighty mass of the sun, such the prodigious supply of heat, or what is equivalent to heat, which it contains, that the grand transformation through which it is passing proceeds at a rate so slow that, during the ages accessible to our observations, the results achieved have been imperceptible. Think of a sphere the size of the earth. Would it be possible to detect the curvature of a portion of its equator a yard in length? To our senses, nay, even to our most refined measurements, such a line, though indeed a portion of a circular arc, would be indistinguishable from a straight line. So is it with the solar radiation. To our ephemeral glance it appears to be quite uniform; we can only study a very minute part of the whole series of changes, so that we are as little able to detect the want of uniformity as we should be to detect the departure from a straight line of the arc of a circle which we have given as an illustration.

We cannot, however, attribute to the sun any miraculous power of generating heat. That great body cannot disobey those laws which we have learned from experi-

ments in our laboratories. Of course no one now doubts that the great law of the conservation of energy holds good. We do not in the least believe that because the sun's heat is radiated away in such profusion that it is therefore entirely lost. It travels off no doubt to the depths of space, and as to what may become of it there we have no information. Everything we know points to the law that energy is as indestructible as matter itself. The heat scattered from the sun exists at least as ethereal vibration if in no other form. But it is most assuredly true that this energy so copiously dispensed is lost to our solar system. There is no form in which it is returned, or in which it can be returned. The energy of the system is as surely declining as the store of energy of the clock declines according as the weight runs down. In the clock, however, the energy is restored by winding up the weight, but there is no analogous process known in our system.

It was long a mystery how the sun was able to retain its heat so as to supply continually its prodigious rate of expenditure. The suppositions that would most naturally occur were shown to be utterly insufficient. We know that a great iron casting often takes many hours to grow cold after it has been drawn from the mould. If the casting be a sufficiently large one, the cooling will proceed so slowly that it will not get cold for days, because the tardiness of cooling increases with the dimensions of the body. It was not, perhaps, unnatural to suppose that as the sun was so vast, the process of cooling would proceed with such extreme slowness that notwithstanding the quantity of heat poured out every second, the annual amount of loss would be so small relatively to the whole

store that the effect of that loss would be imperceptible in such periods as those over which our knowledge extends. This supposition, however plausible, is speedily demolished when brought to the test by which all such questions must be decided—the test of actual calculation.

We can determine with all needful accuracy the store of heat that the sun would contain if regarded merely as a white-hot solid globe. When we apply the known annual loss, we see at once that if the sun had merely the simple constitution here supposed, the annual expenditure would bear such a considerable proportion to the total supply that the effect of the loss would become speedily apparent. It is certain that the sun must under such circumstances fall some degrees in temperature each year. In a couple of thousand years the change in temperature would be sufficiently great to affect in the profoundest manner the supply of sunbeams. As, however, we know that for a couple of thousand years, or, indeed, for periods much longer still, there has been no perceptible decrease in the volume of solar radiations, we conclude that the great luminary cannot be regarded merely as a glowing solid globe dispensing its heat by radiation.

There is another supposition as to the continuance of sun heat which must be mentioned, only, however, to be dismissed as quite incapable of offering any solution of the problem. As we generate heat here so largely by the combination of fuel, it has been sometimes thought that a similar process may be in progress on the sun. It has been supposed that elements capable and desirous of chemical union may exist in the sun in such profusion that by their entering into association a quantity of heat is liberated sufficient to account for the continuous dispersal by radiation.

Here, again, the test must be applied which is decisive of such pretensions. It may certainly be the case that chemical actions of one kind or another are going on in the sun, and among them are doubtless some of such a character that they evolve heat. But we happen to know exactly how much heat can be evolved by the action of specified quantities of elementary bodies by whose union heat is generated. It appears clear from the figures that chemical action is a wholly inadequate method of accounting for solar radiation. To take one instance, we may mention that if the sun had been a globe of white-hot carbon, and if there had been a sufficient supply of oxygen to effect its combustion, the total heat generated by the entire mass would not supply the solar radiation for the period that has elapsed since the building of the pyramids. It is, therefore, clear that the supposition that the sun is a burning globe, like the supposition of the sun as a cooling solid globe, is quite inadequate to explain the marvellous persistence with which, for countless ages, the orb of day has distributed its beams.

There is another supposition which, though not itself providing the explanation that we are searching for, still points so far in that direction that I have kept it till the last. It has been sometimes suggested that the dashing of meteoric matter into the sun from outside may afford the requisite supply of energy. There can be no doubt that the plunge of a meteor into the sun's atmosphere with the terrific velocity which it will necessarily acquire in consequence of the attraction of the sun, is accompanied by the transformation of the energy of the meteor's movement into light and heat. The quantity of energy that a meteor thus carries with it is so vast that

it is hardly credible until the figures which express it and the grounds on which they are based have received due attention. Let us think of a meteor which is moving, as such bodies do when near the earth, with a speed perhaps a hundred times as great as that of a bullet from a rifle, or even from one of the most finished pieces of artillery. The energy of the meteor, depending as it does upon the square of the velocity, will be, therefore, about ten thousand times that of the bullet of the same size. It seems that the energy thus possessed by a meteor one pound in weight is as much as could be developed by the explosion of a ton weight of gunpowder. Doubtless, in the vicinity of the sun, the meteors are more numerous, and they move with a vastly higher velocity than the meteors near the earth. It is therefore plain that the quantity of energy contributed to the sun from this source must be very large in amount. It can, however, be shown that there are not enough meteors in existence to supply a sufficient quantity of heat to the sun to compensate the loss by radiation. The indraught of meteoric matter may indeed certainly tend in some small degree to retard the ultimate cooling of the great luminary, but its effect is so small that we can quite afford to overlook it from the point of view that we are taking in these pages.

It is to Helmholtz we are indebted for the true solution of the long-vexed problem. He has demonstrated, in the clearest manner, where the source of the sun's heat lies. It depends upon a cause which, at the first glance, would seem an insignificant one, but which the arithmetical test, that is so essential, at once raises to a position of the greatest importance. It is sufficiently obvious that the sun is in no sense to be regarded as a

solid body. It seems very unlikely that there can be throughout its entire extent any portion which possesses the properties of a solid; certainly those exterior parts of the sun which are all that are accessible to our observation are anything but solid: they are vast volumes of luminous material floating in gases of a much less luminous nature. The openings between the clouds form the spots, while the mighty projections which leap from the sun's surface testify in the most emphatic manner to the gaseous or vaporous character of the outer parts of the great luminary. A gaseous globe like the sun when it parts with its heat observes laws of a very different type from those which a cooling solid follows. As the heat disappears by radiation the body contracts; the gaseous object, however, decreases in general much more than a solid body would do for the same loss of heat. This is connected with a striking difference between the manner in which the two bodies change in temperature. The solid, as it loses heat, also loses temperature; the gas, on the other hand, does not necessarily lose temperature even though it is losing heat. Indeed, it may happen that the very fact that the gaseous globe is losing heat may be the cause of its actually gaining in temperature and becoming hotter. This seems a paradox at the first glance, but it will be found not to be so when due attention is paid to the different notions that belong to the words heat and temperature. The globe of gas unquestionably radiates heat and loses it, and the globe, in consequence of that loss, shrinks to a smaller size. The heat, or what is equivalent to heat, that is left in the globe, is exhibited in a body of reduced dimensions, and in that smaller body the heat shows to such advantage that the

globe actually exhibits a temperature hotter than before the loss of heat took place.

In the facts just mentioned we have an explanation of the sustained heat of the sun. Of course we cannot assume that in our calculations the sun is to be treated as if it were gaseous throughout its entire mass, but it

Fig. 32.—Solar Eruption, May 3, 1892.

approximates so largely to the gaseous state in the greater part of its bulk that we can feel no hesitation in adopting the belief that the true cause has been found. To justify the adequacy of this method of explaining the facts I may mention the following result of a calculation. If the sun were to lose sufficient heat to enable it to shrink in its diameter by one ten-thou-

sandth part of its present amount, the quantity of heat that would be available in consequence of this contraction would suffice to provide the entire radiation for a period of 2,000 years. Such a diminution of the sun's bulk would be altogether too small to be perceptible by the most refined measurements that we can make in the observatory. Hence we are able to understand how the prodigious radiation of the sun during all the centuries of history can be accounted for without any alteration in the dimensions of the great luminary having yet become appreciable.

But there is a boundary to the prospect of the continuance of the sun's radiation. Of course, as the loss of heat goes on, the gaseous parts will turn into liquids, and as the process is still further protracted, the liquids will transform into solids. Thus we look forward to a time when the radiation of the sun can be no longer carried on in conformity with the laws which dictate the loss of heat from a gaseous body. When this state is reached the sun may, no doubt, be an incandescent solid with a brilliance as great as is compatible with that condition, but the further loss of heat will then involve loss of temperature.

At the present time the body may be so far gaseous that the temperature of the sun remains absolutely constant. It may even be the case that the temperature of the sun, notwithstanding the undoubted loss of heat, is absolutely rising. It is, however, incontrovertible that a certain maximum temperature having been reached (whether we have yet reached it or not we do not know), temperature will then necessarily decline. There is certainly no doubt whatever that the sun, which is now losing heat, even if not actually falling in temperature, must at some

time begin to lose its temperature. Then, of course, its capacity for radiating heat will begin to abate. The heat received by the earth from the great centre of our system

Fig. 33.—Solar Eruption, April 8, 1892.

must, of course, decline. There seems no escape from the conclusion that the continuous loss of solar heat must still go on, so that the sun will pass through the various stages of brilliant incandescence, of glowing redness, of

dull redness, until it ultimately becomes a dark and non-luminous star. In this final stage the sun will only pass into that state in which the majority of the bodies in the universe are already found. Every analogy would teach us that the dark and non-luminous bodies in the universe are far more numerous than the brilliant suns. We can never see the dark objects, we can discern their presence only indirectly. All the stars that we can see are merely those bodies which at this epoch of their career happen for the time to be so highly heated as to be luminous.

There is thus a distinct limit to man's existence on the earth, dictated by the ultimate exhaustion of the sun. It is, of course, a question of much interest for us to speculate on the probable duration of the sun's beams in sufficient abundance for the continued maintenance of life. Perhaps the most reliable determinations are those which have been made by Professor Langley. They are based on his own experiments upon the intensity of solar radiation, conducted under circumstances that give them special value. I shall endeavour to give a summary of the interesting results at which he has arrived.

The utmost amount of heat that it would ever have been possible for the sun to contain would, according to this authority, supply its radiation for 40,000,000 years at the present rate. Of course, this does not assert that the sun, as a radiant body, may not be much older than the period named. We have already seen that the rate at which sunbeams are poured forth has gradually increased as the sun rose in temperature. In the early times the quantity of sunbeams dispensed was much less per annum than at present, and it is, therefore, quite possible that the figures may be so enlarged as to meet the requirements

of any reasonable geological demand with regard to past duration of life on the earth.

It seems that the sun has already dissipated about four-fifths of the energy with which it may have originally been endowed. At all events, it seems that, radiating energy at its present rate, the sun may hold out for 4,000,000 years, or for 5,000,000 years, but not for 20,000,000 years. Here then we discern in the remote future a limit to the duration of life on this globe. We have seen that it does not seem possible for any other source of heat to be available for replenishing the waning stores of the luminary. It may be that the heat was originally imparted to the sun as the result of some great collision between two bodies which were both dark before the collision took place, so that, in fact, the two dark masses coalesced into a vast nebula from which the whole of our system has been evolved. Of course, it is always conceivable that the sun may be re-invigorated by a repetition of a similar startling process. It is, however, hardly necessary to observe that so terrific a convulsion would be fatal to life in the solar system. Neither from the heavens above, nor from the earth beneath, does it seem possible to discover any rescue for the human race from the inevitable end. The race is as mortal as the individual, and, so far as we know, its span cannot under any circumstances be run out beyond a number of millions of years which can certainly be told on the fingers of both hands, and probably on the fingers of one.

CHAPTER XII.

THE "HEAT WAVE" OF 1892.

DURING the course of the summer of 1892 the papers frequently described in sufficiently striking paragraphs the abnormally high temperature which was experienced in many parts of the globe. The first tidings of this nature reached us from America. Thus we read that on the 29th of July the thermometer in the streets of New York had risen to as much as 101° and 102° in the shade. At the meteorological station in that city, where, no doubt, every precaution was adopted to insure accuracy in the record, we find that a temperature of 99° was indicated. The next day—July 30—the ascent of the mercury still continued, and we hear that an observation in the Fifth Avenue showed as much as 107° in the shade. This, however, seems to have been the culmination of what had been somewhat absurdly designated "the great heat-wave." On July 31 the warmth had begun perceptibly to decline, though it was still terribly oppressive.

The descriptions received from various parts of the North American continent show that the heat was almost,

if not quite, as great in many other places as it was in New York. From north and south, from east and west, we heard of abnormally high thermometers; we were told that in many localities the work in factories had to be discontinued, as the hands could not stand the heat. In some towns business seems to have been temporarily suspended, and the traffic in the streets ceased during the hottest part of the day. It was also reported from many places that heavy losses were experienced by the death of sheep and cattle. Nor was the great heat-wave without a tragic aspect. We read of a large number of cases of sunstroke occurring in various parts of America, many of which terminated fatally.

So far as we are able to form a picture of what actually happened, it would seem to have been one of the most protracted and calamitous spells of heat that have ever been recorded in America. It has been remarked as a somewhat peculiar feature, that there was an almost total absence of wind at the time when the heat was greatest; and it may also be recorded that the air was at the time largely charged with humidity. Every one who has had any experience of tropical heat knows that the suffering caused by an excessively high temperature is greatly enhanced if the air be saturated with moisture. Evaporation is then almost at a standstill, and one of the means by which the temperature of the body is kept down is so far rendered inoperative. I recollect being told by an officer who was in the Ashantee Expedition many years ago, that notwithstanding the excessive heat of the coast off which their ships lay at anchor, there was practically no evaporation, owing to the air being saturated with moisture. The towels which were hung up

to dry in the morning remained wet till evening, even though the tropical sun beat on them all the day long. Heat of a somewhat similar character appears to have been experienced in America at the end of last July.

It was about a fortnight or three weeks after the New World had its scorching that the Old World was visited by the great heat-wave. Up to the beginning of August there does not seem to have been anything unusual in European temperatures; thus, for instance, at Berlin, on August 1, the highest thermometric reading was 72°, and the lowest 61°. Even on the 7th of August, the greatest and least temperatures at Vienna were no more than 70° and 61° respectively, but towards the middle of the month the ascent of the mercury in the thermometer became marked and rapid all over Europe.

By the 17th of August, a temperature had been reached at Vienna which seems to have rivalled that attained at New York nineteen days previously. We read that on the following day (18th of August) the thermometers at Vienna showed 107° in the shade; the telegrams declare that the streets are deserted, and considering what the feelings of the reporter must have been who described it, we excuse his exaggeration that the Ringstrass was "like a furnace."

On the 19th, Berlin is reported to be almost unbearable, and, on the same day, we read that the heat is tropical at Paris, where there have been many fatal cases of sunstroke. It is further stated that 100 oxen and 300 pigs were found dead from the heat in the railway trucks as they arrived in the meat-market at Villette.

On August 22, the phase described in the papers as "almost unbearable" is recorded at Vienna, and that this

language is justified will be obvious from one fact which is mentioned in the same connection. It appears that a body of troops which were out for manœuvres in the neighbourhood of Vienna during this terrible weather, suffered so severely that there were 200 cases of sunstroke among them, and many of those so attacked did not recover. About the fourth week in August, England experienced in some small measure the effects of the great heat-wave. But only in small measure, because we happen to lie on the margin of the globe area which was the seat of the high temperature. However, it may be remarked that for two or three days, an unusually high thermometer prevailed in South-Eastern England. On August 24, 80° is recorded in the shade at Dover, and on August 23 and 24, the highest and lowest indications of the thermometer at London were 80° and 59° respectively. It follows that the temperatures attained in this country fell far short of what was experienced in so many places on the Continent, nor did the unusual heat which was reached last long in Great Britain. We find that by the 24th and 25th of August, the range at London had so far declined that the highest and lowest points were 75° and 62° respectively. It was not till some days later that the decline really set in on the Continent; for on the 25th of August, there was still a temperature of 89° in the shade at Vienna. On the 26th of August, which is the last record of the great wave that we shall here set down, the thermometer shows 84° at St. Petersburg, the report accompanying it with the emphatic word "scorching."

From the various facts we have set forth it appears that towards the end of July an extraordinarily high temperature, even for that period of the year, prevailed over a

very large part of the North American continent. The so-called heat-wave then seems to have travelled eastward, and crossed the Atlantic Ocean, but, owing to the absence of information, except in such casual records as may be found by an inspection of ships' logs, we know little or nothing of the actual progress of the heated region across the Atlantic. However it may have come about, it is, at all events, certain that a fortnight after the occurrence of unusually great heat in the New World there was a similar experience in the Old World. Our knowledge of the distribution of temperatures over the whole globe is too incomplete to enable us to follow the movements of the great wave as fully as we might desire. No doubt our own Meteorological Office does most admirable work, and of course many other countries have more or less complete organisations for the study of meteorological phenomena, yet our information as to the thermal condition of the globe still falls short of what we would like to have. Certain materials are, however, available, and we shall endeavour to throw what light we can on the matter.

We often hear the question asked as to what was the cause of this exceptional heat. Let me hasten to say that neither in these pages nor anywhere else could I attempt to answer this question in the sense in which it has usually been proposed. It is very doubtful whether it would be possible to assign a single cause for such a phenomenon, even if we knew many things of which we are now completely ignorant. Indeed the most difficult problem of astronomy becomes simplicity itself when compared with the extraordinarily complex agents that are in operation even in the simplest meteorological phenomenon. Let me illustrate this contrast between the two sciences by an example.

The movement of the moon is one of the most profound dynamical problems. It depends principally on the attraction of the earth, and in a lesser degree on the disturbance caused by the sun. The forces thus arising can be submitted to calculation, and though the work involved is extremely abstruse, and though it implies a prodigious amount of numerical labour, yet it can be completely solved for all practical purposes. The consequence is that the motions of the moon have become so well known that we can foretell not only the hour but even the minute at which eclipses will occur next year or in a hundred years to come. Contrast the certainty of this knowledge with the vagueness of our knowledge of meteorological phenomena. We can tell you precisely where the moon will be at noon next Christmas Day, or, for that matter, where the moon will be at noon on Christmas Day in the year 1994. But who can tell what the temperature will be at noon next Christmas Day on London Bridge? No scientific man could venture on such a prophecy. He knows that he has no data to go by. The number of causes which are in operation is so great that the problem becomes of a highly complex nature. There is, however, a certain mathematical principle which applies in this case. It does not, indeed, enable us to predict the actual amount of any meteorological element, but it appears to demonstrate with all desirable fulness that there must be definite laws governing the changes of the different meteorological elements if only we were able to discover them.

The argument on which we are about to enter is perhaps a somewhat difficult one, but it will be worth while to face it. The method indicated seems to offer the only hope of our ever attaining such a knowledge of meteoro-

logical phenomena as will enable us to rise to the supreme position of being able to predict the facts of climate with assured accuracy, and for a long time in advance. Let us first enumerate some of the particular phenomena which are necessarily more or less connected together.

The most fundamental of all the elements concerned is the pressure of the air as indicated by the barometer; then there is the temperature of the air and the degree of its saturation, the amount and character of the clouds, the rainfall, together with comparatively exceptional incidents such as hailstorms and thunderstorms. At present, no doubt, we are enabled, by the careful collection of observations all over the world, to predict in some degree the recurrence of these phenomena. Our newspapers give us each morning a forecast of the kind of weather that may be expected. But every one knows that, though these forecasts are often useful, they yet have a very inferior degree of accuracy to the kind of prediction which we find in the "Nautical Almanac," where the occurrence of an eclipse of the moon, or of an occultation of a star, or a transit of Venus, or any similar astronomical event, is foretold with definiteness and with perfect certainty of fulfilment. Yet no one can really doubt that the temperature at London Bridge next Christmas Day, or the height of the barometer at noon on January 1, 1900, are each of them quite as certainly decided by law as the time of high water or any other astronomical element.

We know that there will be a transit of Venus in the year A.D. 2004, and that there will be no such phenomenon until then, while there will be a repetition of the occurrence in A.D. 2012. It is certain that these predictions will be fulfilled, yet why is it that we can

make no assertion of a similar character with regard to the meteorological phenomena? The one is just as amenable to law as the other, but the difference is that the extreme intricacy of the causes which affect the meteorological phenomena have hitherto prevented us from discovering the laws by which they are regulated.

Perhaps the differences between the state of our knowledge of the astronomical and of the meteorological phenomena will be more conveniently explained by choosing a branch of astronomical science with which we are at present only imperfectly acquainted. Let us take, for instance, the showers of shooting stars, which are wont to occur on November 14–16. Every one knows that there was a superb display from this shower in 1866, and there were some reasons to expect that there might also be a superb display in 1899. As will be well remembered, this expectation was not realised, so we only cite the illustration to point out that there are certain kinds of expectation which stand on a very different plane from astronomical predictions of a more legitimate character.

Nor is it hard to see the reason why this is so; we know in a general way the orbit of that swarm of bodies whose incursions into our atmosphere give us shooting-star showers. There are, however, many circumstances in connection with the movements of these little objects, which as yet are only imperfectly disclosed to us. We have no very accurate knowledge as to the manner in which the shoal of meteors is disposed around the vast ellipse which constitutes its path, and consequently our predictions must necessarily be put forth with some feeling of insecurity. It is quite certain no doubt that the earth crosses the track of the meteors every November

12—14, and it may also be regarded as tolerably certain that when the earth is in this position in the year 1899 the shoal of little bodies will be in our vicinity. We believe that the earth will actually pass through the shoal, in which case a great meteoric display will be the result—if the weather permits! It may, however, happen that we shall only traverse a sparsely occupied portion of the great host, in which case the shower will fall much short of others which have been recorded. An enormous volume of quite unattainable knowledge would have to be at our disposal were we to be able to predict with certainty all the circumstances of such phenomena; we should have to know exactly what meteors there were in the shoal, and the dimensions and other features of the orbit which every single meteoroid followed. If such knowledge as this were possible, then the future circumstances of the shower might be predicted with almost as much accuracy as the announcement of the next eclipse or the next opposition of Mars.

This illustration will suffice to explain the reasons why our knowledge of meteorological phenomena is at present in such an imperfect state as compared with those of astronomy. The supreme test of the completeness of any physical theory is the successful prediction of results—we are not yet able to predict great heat-waves or great storms with any assured confidence, not because such phenomena do not observe definite physical laws, but because the knowledge that we should require before we could exactly specify these laws is in a great measure wanting. We are, however, not without grounds for encouragement in the belief that the time may yet come when the definite prediction of meteorological phenomena

may become possible. An instructive illustration of the direction in which we may look for success is afforded by the study of the tides. Of late years the problem of tidal prediction has occupied a great deal of attention, and by the labours of Lord Kelvin, Sir George H. Darwin, and others, the investigation has received a completeness which renders it a typical example of how the solution of a problem of this kind is to be attained.

If we are ever to progress in meteorological prediction we can only do so by following the same lines which have already been pursued with striking results in the case of the tides. Of course the tides primarily depend on the attraction of the moon, but to a secondary extent the great undulations of the ocean are affected by the influence of the sun. As the movements of both these heavenly bodies may be regarded as sufficiently known, the matter of tidal prediction would be indeed a simple affair were there no other element to be taken into consideration. But the time of high water at any port as well as the actual height which the water attains are by no means regulated solely by the positions of the sun and moon ; it is the configuration of the surrounding coasts, the depths of the seas in the neighbourhood, the proximity or the remoteness of the open ocean, and other purely local circumstances which affect the result—all these have to be taken into account.

The most instructive method of exhibiting the present state of tidal theory is given by Lord Kelvin's tide-predicting machine. In this arrangement the difference between what we may call the astronomical factors and the terrestrial factors of the tides, is clearly brought out. A cord passes over a number of pulleys and the

centres of each of these pulleys are made to revolve in periods which are determined by the movements of the sun and moon. When the machine is to be employed for predicting the tides in any particular port, the positions of all these pulleys must be set so to speak in conformity with certain individual circumstances connected with the particular port—thus, though the tides at Madras are totally different from the tides at London Bridge, yet the same machine may be used to calculate both. The fundamental movements of the machine are constant for all ports, but the various pulleys will in the cne case have to be set in conformity with the local conditions of Madras, and in the other case they would have to be set in conformity with the local conditions of London Bridge. Two totally distinct tide-tables, appropriate however to the two ports named, could thus be generated by the revolutions of this useful machine.

It would perhaps be too much to anticipate that the time will ever come when meteorological phenomena shall admit of being worked out by a machine on the principle of the tide-predicting engine. But yet it does not seem altogether vain to strive for such a result. We can, in fact, give some reasons for indulging a hope that something of this kind may yet be accomplished. In the first place it is perfectly clear that the radiation of heat from the sun must be the chief factor in the variations of all meteorological quantities. The fluctuations of temperature with the changing seasons are among the most obvious instances of the connection between the sun and the climate, but it may be shown that the changes of every other meteorological element are also primarily dependent on the sun.

Let us take, for instance, the pressure of the air as indicated by the height of the barometer, and show that the oscillations of the mercury must be due to the sun. Imagine for a moment that the sun were to be extinguished, one consequence of the cessation of the arrival of heat at the earth would be that winds would blow no longer. There could be hardly any movements whatever in the air except such as might arise from atmospheric tides. Perhaps also I ought to add that the internal heat of the earth as manifested by earthquakes or by occasional volcanic outbreaks might produce some local and temporary disturbance of the air. It is, however, quite certain that such influences would have very slender effects on atmospheric pressure. The argument will at all events suffice to show that the fluctuations of the barometer to which we are accustomed are almost entirely attributable in one way or another to the action of the sun. It can similarly be shown that the changes in every meteorological element will depend primarily upon the great luminary. In some cases, of course, the rotation of the earth on its axis is also an important element, and to some minute degree the moon must be reckoned with. But when these influences have been considered, we have no further concern with the heavens; it is the topographical features of the earth which complete the determination of all meteorological quantities.

I cannot here go into the discussion of a celebrated mathematical theorem which bears the name of Fourier. It seems, however, to demonstrate that any meteorological element, such as the height of the barometer or the temperature, must admit of being expressed in a somewhat similar fashion to the height of the tide. No doubt the

arrangement of pulleys would have to be extremely complex, so as to enable the elements to be determined which were dependent upon so many considerations. It is, however, quite plain that if we are ever to succeed in subjecting meteorological phenomena to numerical precision it must be in some such direction as I have indicated. To put the matter a little more plainly; we have reason to believe that a system of pulleys could be so arranged, and the relative movements of them could be so actuated, that a cord passing over those pulleys and adjusting a pencil could be made to show the height of the barometer for every day in the year at a given place. A similar machine might also be conceived which should show the temperature at any stated locality for every hour in the year. I do not for a moment assert that the information at present at our disposal would enable us to construct such machines. All I am now contending for is that mathematical theory seems to declare the possible realisation of such contrivances. The fact that an engine has already been constructed for the comparatively simple case of the tides leads us to hope that the time may arrive when meteorological engines shall have been designed by which meteorological prediction shall become as determined as the prediction of high water.

This discussion will at all events enable us to make some reply to the question which has been often asked, as to what was the cause of the great heat-wave. I do not indeed think that the question admits of any off-hand answer of the kind that is frequently expected by those who ask it—the only kind of answer that seems possible is of a somewhat indirect character. We may here again revert to our illustration of the tides. It sometimes hap-

pens that an unusually high tide occurs. In the port of Dublin, for instance, we have had from time to time exceptionally high water in the Liffey, which has flooded the basement stories of low-lying dwellings. The cause of such extraordinary phenomena is not to be attributed to any unusual development of the strength of the moon's tide-producing capacity, it is rather to be explained in a manner which the tide-predicting engine renders easy to understand. Besides the main lunar tide and the main solar tide, there are several minor tides, so to speak, arising from the different combinations of the movements of the sun and moon. Each pulley in the tide-predicting engine is, in fact, allocated to each particular tide—the consequence is, that the height of the water at any moment is the net result of one or two large tides, and of a number of small ones; thus, for instance, every one knows that the spring tides, as they are called, are exceptionally high because the sun and moon conspire; while the rise and fall at the time of neap tide are comparatively small, because then the solar tides act oppositely to the lunar tides, and what is actually perceived is only the difference between the two. There are also the numerous minor tides to be considered; of course, it will not generally happen that these are all consentaneous: some of them are high and some of them are low, and others may be at intermediate phases at the time of high water, as determined by the great predominating tide. But it is easy to imagine that every now and then, under exceptional circumstances, there will happen to be a concurrence between the time of high water in the small tides and in the great ones. Then, of course, there will be the exceptional flooding that is occasionally experienced.

It is somewhat in this manner that we must seek to explain what is called the great heat-wave. The temperature at any place has of course the main annual period corresponding to the variation with the seasons, but there are many other periodic fluctuations in the temperature analogous to those minor tides to which we have already referred. Generally speaking, of course, these will not all conspire; some will be tending to elevate the temperature and others to depress it at any time. It is the net result of all that we actually perceive. Sometimes, however, it will happen that several of these, or at all events some of the more important ones, move in the same direction; then of course we have great exaltation of temperature such as that which the newspapers have called the great heat-wave.

It is, however, quite possible that certain changes in progress on the sun may act in a specific manner on our climate. I do not indeed say that there is much reason for thinking that the great heat-wave has really been connected with any intrinsic changes in our luminary, but it is just possible that something of the kind may have occurred. I would consequently like to devote some little space to the consideration of this interesting subject.

In the discussion of such a question there is a fundamental point which must always be borne in mind. We must remember the full extent of the earth's indebtedness to sunbeams. We have spoken of a temperature of 100° during the continuance of the great heat-wave, and it is necessary to understand all that this implies. Of course, on our thermometric scale a temperature such as we have mentioned merely means 100° above a certain arbitrary zero, but the sun has sent us more heat than those hundred

degrees express. If the sunbeams were totally intercepted, so that the earth derived no heat whatever from this source, the temperature of our globe would fall not merely to zero, but it must sink down to a point far below zero, even to the temperature of space itself; what this may be is a matter of some uncertainty, but from all the evidence attainable it seems plain that we may put it at not less than 300° below zero. It therefore follows that at the time of the heat-wave, when the thermometer indicated 100°, the sun's beams actually maintained the affected region of the earth at 400° above what it would have been if the sun were absent. This will show us that the heat-wave was not after all such a very exceptional matter so far as the sun was concerned. Had the temperature been only 80° at New York we should never have heard of the sun-strokes and all the other troubles; it was the extra 20° which made all the difference—in other words, so long as the sun merely kept the earth about 380° above the temperature of space no one thought anything about it, but the moment it rose to 400° it was expected that something tremendous must have happened. This way of looking at the matter places the great heat-wave in its proper cosmical perspective; it was no such great affair after all; it merely meant a trifling addition of 5 per cent. to the temperature usual at that season—that is to say, when the temperature is measured in its proper way. This shows us that a very trifling proportional variation in the intensity of the sun's radiation might be competent to produce great climatic changes. It seems hardly possible to doubt that if, from any cause, the sun shed a small percentage of heat more than it was wont to do, quite disproportionate climatic disturbances would be the result.

It cannot be denied that local if not general changes in the sun's temperature must be the accompaniment of the violent disturbances by which our luminary is now and then agitated. It is, indeed, well known that there are occasional outbreaks of solar activity, and that these recur in a periodic manner; it is accordingly not without interest to notice that the present year has been one of these periods of activity. We are certainly not going so far as to say that any connection has been definitely established between a season of exuberant sun-spots and a season remarkable for excessive warmth, but, as we know that there is a connection between the magnetic condition of the earth and the state of solar activity, it is by no means impossible that climate and sun-spots may also stand in some relationship to each other.

As to the activity of the sun during the hot summer, a very striking communication was made at the time by one of the most distinguished American astronomers, Professor George E. Hale. He has invented an ingenious apparatus for photographing on the same plate at one exposure both the bright spots and the protuberances of the sun. Professor Hale delivered an interesting lecture at a meeting of the American Association for the Advancement of Science at Rochester. A report of this lecture appeared in *Nature*, in which we are told of a remarkable application of Professor Hale's apparatus. On the 15th of July a photograph of the sun showed a large spot. Another photograph taken in a few minutes exhibited a bright band; twenty-seven minutes later a further exposure displayed an outburst of brilliant faculæ all over the spot. At the end of an hour the faculæ had all vanished, and the spot was restored to its original condition. It was not

a mere coincidence that our magnetic observatories exhibited considerable disturbance the next day, and that brilliant auroras were noted. The whole communication was of such an interesting description that we are not surprised to hear that a vote of thanks was passed to Professor Hale amid much enthusiasm. It is quite plain that we have yet much to learn concerning the effect of the sun on things terrestrial. This new method, in which Professor Hale has extended and improved the processes of his predecessors, is full of hope for the future.

CHAPTER XIII.

VISITORS FROM THE SKY.

METEORITES have quite a peculiar claim on the attention of the astronomer. No doubt the spectroscope provides him with the means of demonstrating inferentially the existence of iron, and of many other terrestrial elements both in the sun and in many of the stars. But he is actually able to handle something which has come from the outside to the earth when he is in possession of a meteorite. Then, too, the movements of meteorites have a certain significance both for the astronomer and the mathematician. It is true that only the last stage of its wild career can be observed, for the flight of the meteorite for millions of preceding years is quite unknown, so far as any direct observations are concerned. The observer never beholds one of these bodies until that supreme moment when at the conclusion of its incalculable journey it flashes from the sky and strikes the earth beneath. The history of these objects before they are plunged into our atmosphere can only be ascertained by conjecture. I do not mean to say that we have no definite information by which to guide such conjectures. The laws of

dynamics are fortunately available, and though they do not, it is to be regretted, convey all that we should like to know, yet still they teach us something with regard to the movements of the meteorites, and thus conduce to the solution of the great problem of their origin.

I am fully aware that there is considerable diversity of opinion as to the origin of meteorites. To me, however, it appears that the source of these bodies ought not to be a matter of much uncertainty. At the commencement of the inquiry it will be well to remove what is, I believe, a not uncommon misconception with regard to the character of meteorites. There can be no doubt that one of these bodies is often accompanied in its descent to the earth by a flash of fire, and not unfrequently a loud detonation announces at the same time that some violent disruption of a mass of matter has taken place. To this extent the fall of a meteorite presents phenomena resembling those which often accompany the apparition of a great fire-ball. In fact I suppose it can hardly be doubted that many of the fire-balls not recognised as meteoritic might have let fall mineral masses under somewhat different circumstances of impact upon the earth's atmosphere. But now comes one of the difficulties in the subject.

In general great fire-balls are little more than conspicuous shooting-stars, and there are shooting-stars of every dimension from those which illumine a country side with splendour down to the little streak of light which can only just be discerned as it flashes through the field of a powerful telescope. A noteworthy circumstance which interests the student of shooting-stars is their periodical exhibition in splendid displays. I allude to such a great shower as that of the Leonids which took

place in 1866, and to other showers from the same system which have appeared in such a way as to indicate their recurrence in a period of thirty-three years. It appears perfectly certain that a distinguishing line has to be drawn between those bodies which dash into the atmosphere to be hurled to the earth as meteorites, and

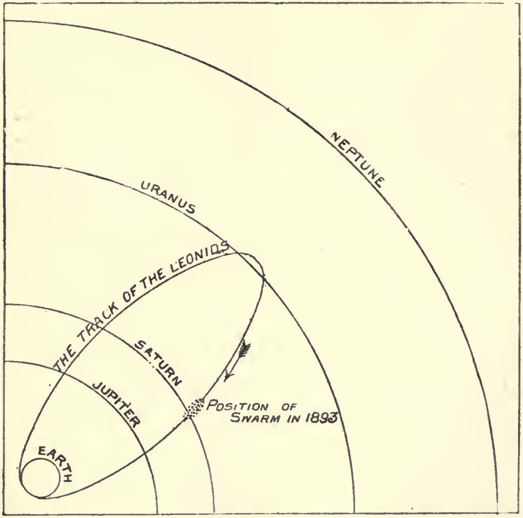

Fig. 34.—The Orbit of the Leonids which produced the Great Shower in 1866.

those shoals of little bodies which constitute the great meteoric showers.

This is, I know, a point about which there is much disagreement among astronomers. Excellent authorities have maintained that there is perfect continuity between

these different classes of objects. Those who hold this view urge that a shower of shooting-stars is merely the aggregation on a grand scale of the isolated or sporadic shooting-stars which are always more or less to be seen, and they affirm that between these occasional shooting stars and the mightiest fire-ball there is a perfect continuity, exhibited by the fact that shooting-stars of every gradation of lustre are from time to time observed. They are thus led to regard meteorites as congenerous with the objects which appear in shooting-star showers. In this inference I am convinced that a serious mistake has been made.

One of the most important results of the great shower of 1866 was the demonstration that the swarm of little bodies to which that shower owed its origin was connected with a comet. The swarm was found in fact to follow the exact track which the comet pursued round the sun. So remarkable a coincidence could not reasonably be accounted for on any other supposition than that the meteors, if not themselves actual fragments of the comet, were at all events so closely connected with it, that they could not have come from any source very different from that in which the comet itself took its rise. This remarkable discovery made with regard to the Leonids may be illustrated and confirmed by similar discoveries of a cometary association in the case of other notable meteoric displays. Probably one of the most remarkable episodes in the whole of this branch of astronomy is connected with Biela's comet. It was known that the body so named revolved in a certain orbit, and a highly dramatic proof was rendered that a shoal of meteors were its fellow-travellers along the same path. As, however, I have discussed this episode

in my work entitled "In Starry Realms," I need not now do more than refer to what has been there said.

Of this connection between cometary orbits and revolving swarms of meteors, many other instances could be cited. I may refer to the remarkable lists published by the British Association, in which, beside the name of the comet or the designation which astronomers had affixed to it, the meteoric swarm with which the comet is associated is also given. The day of the year on which the earth crosses the track pursued by the comet is the day on which the shower of meteors appropriate to that comet is to be expected when the proper interval of entire years has elapsed. The position of the "radiant," or point on the heavens from which the meteors appear to diverge, is exactly that point in the sky where the comet would be seen as it approached the point of crossing over the earth's track by an observer stationed at the crossing. When a meteoric shower appears on the day that has been foretold from the attitude of the cometary orbit, and when the radiant from which the shooting-stars of that shower are directed also occupies the precise position on the sky which has been indicated by the comet, it is then impossible to refuse assent to the belief that the meteors and the comet are in direct association, even if we cannot distinguish that one is the cause, and the other the effect.

On these grounds it appears to be perfectly certain that the origin of the shooting stars which appear in swarms cannot be dissociated from the origin of the comets by which those swarms are accompanied. This fact seems to lead to a demonstration of the important truth that meteorites have no affinity whatever with the ordinary

shooting-stars. Whatever may be the nature of the belief entertained with regard to comets in other respects, it seems quite impossible to regard these bodies as composed of materials in anything like the same forms as those which we find in a meteorite. No doubt the mere elements present in comets may be much the same as those from which meteorites are constituted. In illustration of this I need not do more than refer to the single element iron. It seems to have been demonstrated that iron is a constituent of certain great comets. This same element is of course a leading component of meteorites, and it might therefore be contended that to this extent there was an affinity between the two different classes of bodies. It may also be added that sodium has been found both in comets and in meteorites, while a still more striking instance is presented in the case of the element carbon. This remarkable substance often seems to be one of the principal constituents in a comet, in so far at least as the spectra of those bodies may be regarded as indications of the proportions in which the different elements have united to form it. A curious discovery with regard to the composition of meteorites has been the detection of graphite, as well as of carbon in other forms. Indeed, it may be noted as an interesting circumstance that M. Moisson, in his recent investigations of a meteorite from the Cañon Diablo, detected the presence of minute particles of carbon which possessed the hardness of diamonds.

But while we admit all this it seems that the evidence on the other side is far too strong to permit us to regard comets, and meteorites, as derived from the same source, or as standing to each other in any particular relation. It would be easy to over-estimate the significance of

whatever argument may be derived from the similarity in ultimate chemical composition. It must be remembered that one of the most remarkable results of spectroscopic analysis has been to suggest the practical identity as to ultimate composition of the different bodies of the solar system. There is no good reason to believe, so far as spectroscopic evidence is concerned, that there are any considerable number of elements present in the sun, in addition to those elementary bodies with which terrestrial chemistry has made us acquainted. There is no doubt a mysterious indication of some possible element of an extremely light description in the solar corona, for which the name of "coronium" has been suggested; and there is also some element known as "helium" which is believed to be found in the prominences. But the existence of the bodies so designated is still undemonstrated; and even if it were, the presence of the bodies of such a character would not affect our present argument.

It is therefore not surprising that the elementary bodies which have been discovered in the meteorites should resemble those already known on the earth. Nor need it be a matter for astonishment if the materials found in comets should resemble those found elsewhere throughout the solar system. I therefore think that we are warranted in refusing to draw any inference from the fact that iron is present, both in some meteorites and in some comets, with reference to the presumed relationship between these two classes of celestial objects. They both have iron and carbon simply because they both belong to the solar system, where iron and carbon are elements which appear to be widely distributed. No one will be likely to doubt that iron and carbon are both present in

the planet Mars, although it is true that, from the nature of the case, direct spectroscopic evidence cannot be cited

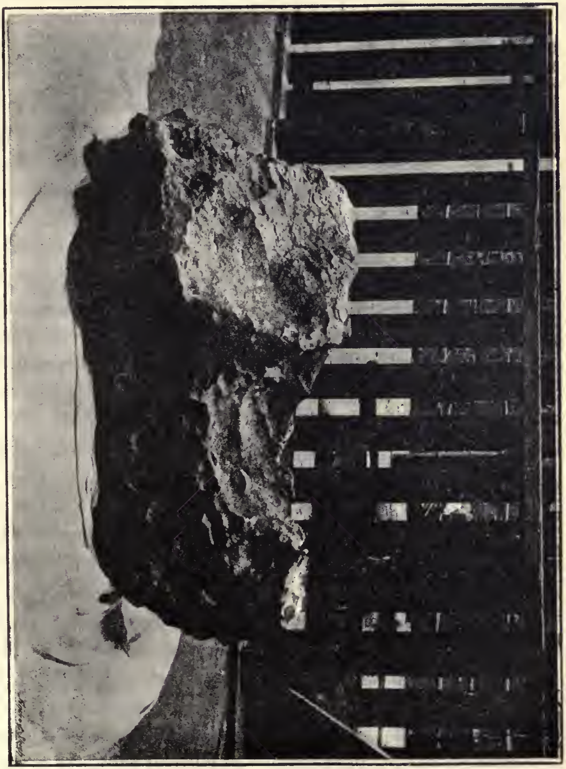

Fig. 35.—The large Youndegin Meteorite. 4 feet 2 inches long, 2 feet 3 inches wide, 20 inches thick. Weight 2,044 lbs. (18¼ cwt.).

on the matter. Nor will it be questioned that iron and carbon are both presumable constituents in the mighty

mass of Jupiter, and yet no one will, on that account, claim that meteorites have been derived from that planet.

The identity of the actual materials in the varied bodies of the solar system is so striking, that it would now seem wonderful if any object which undoubtedly belonged to that system contained materials to any considerable extent not otherwise known to us from their presence in the earth. It appears that there is, therefore, no force in the argument which would connect meteorites with comets, merely because iron and some other substances have been found common to both. On the other hand, the very form in which the iron is found in meteorites, and the condition in which iron must exist in bodies possessing the nature of comets, seems to afford conclusive evidence that the origin of meteorites, and the origin of comets, must be sought for in widely different directions.

Everything we know with regard to the structure or texture of a comet, seems to demonstrate that it is not in the least likely to contain solid masses of which meteorites might be fragments. Take, for instance, the ordinary telescopic comet of which some half-dozen or so come to visit our system every year. Such a body is a light volume of gas or vapour far less dense than the lightest cloud that ever floated in a summer sky. Indeed, it is well known that, as such a comet in its progress across the heavens passes between us and the stars, those stars are often seen twinkling brilliantly right through the many thousands of miles of cometary matter, which their rays have to traverse. The lightest haze in our atmosphere would suffice to extinguish the faint

gleam of these small stars; indeed a few feet of mist would have more power of obstructing the stellar light than cometary material scores of thousands of miles thick.

It is true that the central portions of many of these comets often exhibit much greater density than is found in the exterior regions. Still, in the great majority of such objects, there is no opacity even in the densest part sufficient to put out a bright star. In the case of the more splendid bodies of this description, it may be supposed that the matter is somewhat more densely aggregated as well as more voluminous. Still, however, it will be remembered that the great comet of 1858 passed over Arcturus, and that the star was seen shining brilliantly notwithstanding the interposition of a cometary curtain millions of miles in thickness. So far as I know, no case is known in which the nucleus of a really bright and great comet has been witnessed in the act of passage over a considerable star. It would indeed be extremely interesting to ascertain whether in such a case the star experienced any considerable diminution in its lustre.

There is a delicate method of testing the quantity of matter which a comet might possess in the vicinity of its nucleus. If there were any substantial quantity of gaseous matter, it is plain that though the rays from the star might not be altogether extinguished, or might not even be largely reduced in lustre, yet the apparent direction of the star must be deflected from its course by refraction through the comet. It is well known that when a telescope is used to observe a star, it is not generally pointed exactly in the direction of the star, because the light which it is sought to observe has been bent in its passage through

the atmosphere surrounding the earth, so that the direction in which the light enters the telescope is somewhat different from the direction which the light had when it first encountered the atmosphere at an altitude of some hundreds of miles. The amount of this change in the direction of a ray by refraction is by no means inconsiderable, and the practical astronomer has always to allow for it. At sunset, for example, the light from the departing orb is bent to such an extent, that at the time when the sun has really sunk below the horizon it still appears to be above, on account of the curvature in the rays produced by atmospheric refraction. If, therefore, the light from a star had to traverse in its passage through the comet any quantity of vapour at all comparable in refractive power to the earth's atmosphere, that light would be deflected to an extent which could not be overlooked in the refined methods of modern astronomy.

Here then we have a delicate means for investigating the quantity of refractive matter in a comet. The observations are conducted in this manner. Two neighbouring stars are selected which are so placed with regard to the movements of the comet that it passes over one of the stars while leaving the other uncovered. Suppose that the apparent distance between the two stars upon the sky be measured, before the comet has come into their immediate neighbourhood. The measurement is to be repeated while one of the stars is behind the comet, and a third and concluding observation is to be made after the comet has passed on its way and left both stars behind. We have thus obtained the necessary materials for the investigation. If the body possessed any appreciable refractive power, then the apparent distance of the two stars would

be different in the middle observation of the three, from that which it was both in the first and in the third.

Every practical astronomer knows this is a research which admits of being made with great precision. The angular distance of the two star-like points is an element which the micrometer will indicate accurately to the fraction of

[........................ $5\frac{5}{8}$ inches ....]

Fig. 36.—Crystals of Olivine, embedded in an iron meteorite. Found in the Desert of Atacama, South America.

a second, and if there were a displacement amounting to one hundredth part of that which corresponds to the horizontal refraction of our own atmosphere, the refractive capacity of the comet would be quite unmistakable. By observations of this class it has been shown that there is little or no appreciable refractive power in one of these

vast bodies. These results demonstrate conclusively that the quantity of matter even in a comet is extremely small when compared with its bulk.

The conclusion thus arrived at is confirmed by the fact that our efforts to obtain the weight of a comet have hitherto proved unsuccessful. We have the means of measuring the weight of a planet, by the disturbances of other bodies which it can affect, and if a comet were massive enough to produce disturbances in the planetary movements there would be no difficulty in discovering within certain limits what the cometary mass might amount to. There have been several instances in which a comet has approached so close to a planet, that the attraction between the two bodies must have had significant influence on the planet, if the cometary mass had been at all comparable with that of the more robust body. The most celebrated instance is presented in the case of Lexell's comet which happened to cross the track of Jupiter. The effect upon this body was so overwhelming that it was wrenched from its original path, and started afresh along a wholly different track. The reaction of the comet upon the planet seems, however, to have been incapable of influencing by any measurable quantity the movements of the giant globe. It is, therefore, obvious that the mass of this comet, and it was a large one of its class, was inappreciable in comparison with the mass of Jupiter.

But the rencontre between the two bodies supplies us with an argument of a still more cogent type. The retinue of moons by which Jupiter is attended forms a delicately organized system. Their movements have been observed for centuries, and any derangement introduced into the group by the approach of a considerable foreign body

would be rendered manifest by the disturbance of the little moons from the paths which they had so long traversed. Careful attention was directed to this point in order to see whether, after the collision between Jupiter and Lexell's comet, the satellites offered any indications of the vicissitudes through which they had gone. The evidence on this point was entirely negative. It was not possible to discover any irregularity in the movements of the bodies which could be attributed to the attraction of the comet. It has thus been demonstrated that, notwithstanding the stupendous bulk of a great comet, its mass must have been so inconsiderable as to have been insufficient to disturb even such unimportant members of the solar system as the satellites of Jupiter.

These different lines of reasoning convince us that comets contain no appreciable portion of actual solid material. But meteorites are shown from their structure to be fragments rent from some mighty mass which has cooled but slowly from a highly heated state. They resemble certain volcanic products so closely that it seems quite impossible to refuse assent to the doctrine of Tschermak that, whatever be their source, the materials must have come from gigantic masses, not greatly varying in dimensions from the earth or other solid planets of our system. But we have seen that comets are in every respect different from bodies possessing the characteristics of objects from which meteorites can have been derived. I am, therefore, forced to the conclusion that meteorites and comets can have no connection, except what may be implied in the circumstance that they all belong to the solar system.

I am quite aware that this view is very different

from that which is entertained by distinguished astronomical authorities. For instance, in connection with Sir Norman Lockyer's spectroscopic work, he has been led to frame his meteoric hypothesis in which a comet is represented as containing a cloud of isolated meteorites. For the reasons already given I am unable to assent to this view. The structure of a meteorite seems to be wholly incompatible with the supposition that it had any other origin than as a fragment of some vast mass slowly cooled.

Another distinguished authority, Professor Newton, of Yale College, to whose labours we are so largely indebted for our knowledge of shooting-stars, also considers that a link of connection between meteorites and comets has been established. Indeed, I am aware that this belief is very widely entertained. A specimen of a meteorite has been exhibited in a museum, bearing a label with the words, "a bit of a comet." On the other hand, Professor Lawrence Smith may be mentioned as one distinguished student of meteoric matters who has accepted the view which I have here adopted, namely, that meteorites have no closer connection with ordinary periodic showers or with comets, than they have with Mars, or with Jupiter, or with the sun itself.

It will be remembered that in the early part of this chapter I have insisted on the connection between comets and shooting-star showers, which has, I believe, been abundantly demonstrated. But meteorites seem to be bodies of a radically different character from those meteors which arrive in periodic showers. Meteorites are to be explained on quite different principles; their origin is to be sought in quite different sources. In fact, the only

common bond between objects of such widely different characters is expressed by the fact that they each come into the atmosphere from outside.

If I may say so without offence, it would seem that the logic of the reasoning which connects meteorites with comets is not wholly satisfactory. Some of the arguments which have been brought forward by those who maintain the affinity of meteorites to shooting-star showers appear to be derived from the two following premises. Shooting-star showers come into the air from outside. Meteorites come into the air from outside. But the premises, though both unquestioned, do not admit of our drawing any conclusions as to the affinity of meteorites and shooting-star showers. It is perfectly certain that periodic showers such as the Leonids, or the Perseids, or the Orionids, or the Geminids, or any of the other similar showers, are all cosmical systems possessing distinct affinities to comets. Their origin cannot be discussed separately from the origin of comets. Whence the comets have come, thence these meteors have most probably come, and where that may have been is a question into which I do not now enter. But, besides these bodies, there is another class of objects to which the meteorites belong, which come into our atmosphere, no doubt, but which seem to have no connection with comets. Doubtless there are many of the so-called shooting-stars, or so-called fire-balls, which it would be impossible, with our present knowledge, to assign with certainty to their proper classes. All I am now insisting on is that there are at least two classes of these objects, one of which includes those of cometary affinity, and the other includes the non-cometary objects. It is to the latter that the meteorites belong. This is

proved emphatically by their structure, which is incompatible with the supposition that the body possessing it has originated in a comet.

I would specially commend the important researches of Tschermak to those interested in meteorites. In the museum at Vienna there is a collection of meteorites rivalling our own splendid collection in the Natural History Museum at South Kensington. The eminent Austrian Professor has made an elaborate study of the different bodies of this class in the various museums, and he has come to the conclusion that meteorites have been derived from volcanoes on some large celestial body. So far as I am aware, no other mineralogist has maintained an opinion of an opposite character to that entertained by Tschermak. We may, therefore, inquire where the volcano must have been situated so that the missiles it projects should tumble down on our earth. Placed in this aspect the problem is no longer one for geologists or of mineralogists. It has now come within the province of astronomers and mathematicians. It is for them to say where, in all probability, those volcanoes have been situated from which the meteorites have come. This is indeed a very interesting question, and I propose to undertake its solution.

The missile from a cannon discharged vertically upwards will fall back to earth with a speed nearly as great as that with which it was projected. In fact, the speed at the return would be quite as great as the speed at the departure were it not for the resistance of the atmosphere. Under the actual circumstances of this globe, and with the actual strength of our artillery, and the potency that any available explosives may possess, we are not able

to project a missile with a speed sufficiently great to carry it to a height which would even be an appreciable fraction of the earth's diameter. Our globe is so massive that any velocity which we were able to impart to the upward movement of the bullet would not suffice to carry it to an altitude that bears the same relation to the diameter of the earth as the thickness of an egg shell bears to the diameter of the egg.

It is, however, interesting to consider the circumstances under which a missile would take flight if projected from a globe differing widely from our earth in bulk or mass or physical constitution. Let us first of all suppose that a piece of artillery was to be transferred to some globe much more massive than the earth. Take, for example, some globe possessing the same mass as the sun, and of like dimensions. Under these circumstances, the attraction by which the speed of the ascending missile would be gradually lessened is much more effective than the corresponding force upon the earth. It follows that, even though the missile might leave the mouth of the cannon with the same pace on the large globe that it had on the small one, yet the upward velocity would be abated much more quickly when the heavier mass was underneath, than when it was only the attraction of the smaller of the two globes that was checking the ascent. From the big globe the projectile could not ascend to anything like the altitude which it would be able to attain on the small one, and the time that would be occupied in its flight would undergo a corresponding diminution. Thus, although a projectile might be discharged by a piece of our modern artillery, with a speed sufficient to carry it to an elevation of several miles, yet a like velocity of projection from the

surface of a globe as large as many of the globes in space would only suffice to carry the body to a height of one mile, or even less. It is, therefore, understood that the elevation to which the missile is capable of soaring depends not alone on the efficiency of the cannon from which it has been projected, but on the mass of the globe on which that cannon is placed.

Nor is it indeed only the mass of the globe which is concerned in the matter. It will easily be seen that the diameter of the body must enter as a significant element. Suppose that there were two globes equal in mass, but that in one the materials were of a lighter specific gravity than in the other, and that consequently the globes were of unequal dimensions. Then the attractions of the two globes exercised upon bodies on their respective surfaces would be very different. In the one case the attracted object would be further away from the centre of the globe than in the other, and though the masses of the globes may have been the same yet as the attraction varies inversely as the square of the distance, it would be less on the surface of the greater body than it is on the smaller.

To give an illustration of what I mean, let us suppose the case of two globes equal in weight, but one of which was made of platinum and the other of granite. Platinum is nearly eight times as heavy as granite, it therefore follows that as the globes are of equal weight, the granite globe must be about eight times as bulky as the metallic globe, and this being so it can easily be shown that the radius of the larger globe must be double that of the smaller. There would thus be considerable difference in the gravitation which would

be experienced by the inhabitants of the granite and of the platinum globe respectively. For though the masses of those globes, that is, the quantity of matter they possess, would be the same, yet a denizen of the globe of stone would be twice as far away from the centre of his world as a denizen of the globe of metal would be from the centre of his. So that though the attractions exerted by the two globes at equal distances from their centres would be identical, yet, owing to the law of the inverse square, the inhabitant of the big globe would feel only one-fourth the attraction experienced by the occupant of the small one. It is thus plain that a piece of artillery placed on one of these globes would launch forth its missile under very different conditions from those which would determine the movement if the projection were made from the other globe. As the body left the muzzle the force striving to draw it back to the metallic globe would be no less than four times as great as that which would commence to operate when the missile left the muzzle of the gun on the stone globe. The consequence is that in the latter case the body would ascend to a far greater elevation before its speed was checked and reversed than when it was shot from the platinum world. From this illustration it will be plain that it is necessary to take into account both the mass of a globe and its dimensions when we would determine the height to which a projectile can ascend.

Let us consider the case of a great cannon placed on a globe of comparatively small mass, the density of that globe being about the same as the average density of the worlds which we find in the solar system. It is plain that as the object goes aloft a lessened attractive force will

be exerted to check its upward movement, and consequently the speed will but slowly abate. It, therefore, follows that this force must be put forth for a long time before the upward movement is entirely neutralised, and during this long time the body will have attained a correspondingly high elevation.

It can, indeed, be demonstrated that there is a critical velocity for every globe, such that if a body be projected upwards with that critical velocity or any greater one, it

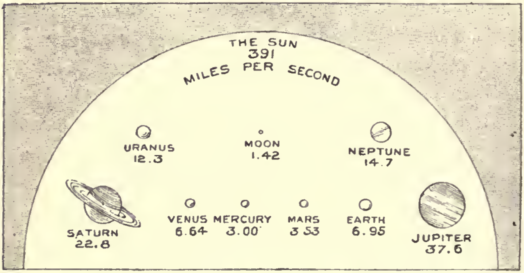

Fig. 37.—Critical velocities in miles per second on the sun and the several planets.

will be permitted to escape altogether. When we know the mass of the globe as well as its radius, we are enabled to calculate what this critical velocity has to be. To take, for instance, the case of the earth. It can be shown that for any globe possessing the weight of the earth, and the exact size of the earth, the velocity with which a missile would have to be shot upwards so as to effect its escape must be about seven miles a second. If the velocity with which the body started on its vertical ascent were less than seven miles a second, then after reaching a height depen-

dent upon the speed of projection it will commence to return. If, however, the pace be as much as or more than the critical value of seven miles a second, the body will continue its journey, and the attraction of the earth will not be sufficient to recall it.

This point is of so much importance that I am inclined to dwell on it a little longer in hope of removing the paradox which it may seem to involve. For as in the ascent of the body the velocity must ever be lessened by the attractive force pulling it back, it might seem obvious that the initial velocity must be ultimately overcome if only long enough time be granted. But this is not a valid objection. It can be shown that the difficulty is something like that which arises in the well-known case of a geometric series. If we add a half to a quarter, and that to an eighth and that to a sixteenth, and that to a thirty-second, and so on indefinitely, we shall make an infinite number of additions, and yet, though the number of the quantities added together may be infinite, the total that they produce can never be more than finite. This can be more simply seen by a very elementary illustration. If you eat half a cake to-day, and a quarter of it to-morrow, and an eighth the next day, and a sixteenth the day after, and a thirty-second the next, and so on, ever consuming one day exactly half what was left the day before, the cake will never entirely disappear. It would, in fact, last for ever, notwithstanding the fact that an infinitely great number of portions had been abstracted from it. Somewhat similar is the nature of the operation by which the attraction of the earth gradually reduces the ascending velocity of the moving body. All that the earth can effect by attraction is to reduce the pace by the extent of seven

miles a second. If the original velocity exceeds seven miles a second, even by the smallest amount, then the attraction of the earth will never be able to overcome it entirely. The projectile would consequently travel outwards into space, never to return to the globe from which it started, unless it should happen that other agents not hitherto contemplated should be brought into operation.

It is known that the energy possessed by a flying bullet is proportional to the square of the velocity with which it is animated. Thus if one of two equal bullets should have a speed double as great as that of the other, then the energy possessed by the more rapidly moving of the two, in consequence of its velocity, will be four times as great as that possessed by the more slowly moving body. It thus appears that a projectile launched with a velocity twelve times as great as that which can be generated by our artillery must possess a quantity of energy one hundred and forty-four times greater than would be acquired by a bullet of equal size fired from a Woolwich gun. We would, therefore, need gunpowder one hundred and forty-four times as potent as any gunpowder we now possess if we expected to shoot a cannon ball away from the earth altogether. Need it be added that the resisting power of the cannon would have to be enormously increased to withstand the stress that such an explosive would generate. In fact, it is perfectly certain that no materials known to us would be strong enough to form a weapon sufficiently tough for the purpose.

If, however, we were performing our experiments on some of the smaller globes belonging to the solar system, then a speed considerably less than that of seven miles a second would suffice to discharge the missile finally from

the vicinity of the globe on which it was placed. A cannon, for instance, which was strong enough to impart to the projectiles that issued from it a velocity double as great as that which our missiles receive here, would send the body entirely away from the moon. On globes still smaller a speed correspondingly less would suffice. Thus in the case of some of the minor planets where the diameters are not expressed in thousands, but only in hundreds of miles, the necessary projectile force would be quite within the range of modern artillery. Indeed, on some of the still smaller globes like the satellites of Mars, where the diameter is a score of miles or less, it is quite likely that velocities comparable with those with which a cricket ball is thrown would suffice to transcend the critical amount. The arm of a vigorous cricketer would cause the ball to ascend with a speed sufficient to make it go on further and further until at last it gradually sank away into the heavens never more to tumble back again to the globe from which its way was sped.

Provided with these considerations, let us now seek to determine the source of meteorites from an astronomical point of view. It is plain that if these objects had been shot as bombs from volcanoes, those volcanoes must have had a potency sufficient to discharge fragments of the solid crust of the globe aloft with such speed that they did not presently return in consequence of that globe's attraction. The great difficulty which we encounter in the consideration of this question arises from the high velocity which would be necessary to set the missiles free from the obligation of immediate return to the parent globe. As the speed of seven miles a second is undoubtedly far in excess of any speed which is attained in terrestrial

volcanoes nowadays, we are tempted to look at some of the smaller globes of the solar system and see whether they can possibly be the abodes of the great volcanoes by whose explosions meteorites have been emitted. We might, at all events, hope by such a supposition to get rid of the initial difficulty with regard to the high speed of projection, for as a low velocity will suffice to carry bodies quite clear of a small globe, it follows that we should not in such a case demand an exceptionally potent volcano.

It has, indeed, been maintained, by at least one distinguished astronomer, that in all probability meteorites are bodies which have taken their departure from volcanoes in the moon. No doubt, so far as the initial velocity is concerned, this supposition would obviate the fundamental difficulty. In the first place, about a fifth of that velocity would be required which would be necessary to discharge a missile with the critical speed from the earth. We know, also, that there is abundant testimony as to the former existence of volcanoes in the moon. That side of our satellite which alone is visible from the earth, is marked over with hundreds of craters, indicating the intense volcanic activity which once reigned there. There can hardly be any doubt that their efficiency may have been ample to impart to the missile a speed sufficiently great. Indeed, the circumstances which we know with regard to Krakatoa seem to show that some of the bombs launched from this volcano in the memorable eruption, issued from the mouth of the crater with a velocity which can hardly have fallen short of a mile a second. In other words, if a volcano like Krakatoa were to break out to-day on the surface of the moon, and if it were to discharge its missiles upwards with velocities comparable with those

[———— $3\frac{3}{4}$ inches ————]

Fig. 38.—Slab of Crystallised Iron cut from the Aerolite of Lenarto, Hungary.

actually observed in the Straits of Sunda in 1883, it is not at all unlikely that many bodies would be shot away from the moon altogether.

So far, however, as the descent of meteorites to the surface of this earth is concerned, it is not alone sufficient to inquire whether these bodies can have left the parent world; it is further necessary to examine the circumstances under which a missile projected from some other globe shall tumble down on this one. This is a point that has been sometimes not sufficiently attended to by those who have considered the matter. It has been thought that as the volcanoes on the moon have been in all probability potent enough formerly, it is therefore reasonable enough to look to them as the source of meteorites. But a little further examination will show that though missiles might undoubtedly have left the moon owing to projection from lunar volcanoes, yet, that

it would be in the highest degree improbable that any of these objects should now be from time to time descending to the earth. For suppose that a meteorite is shot away from the moon, it presently comes so completely under the attraction of the earth that it must revolve about it in accordance with the laws imposed by the earth's gravitation. The question, therefore, as to whether it is to fall on the earth or not, is simply decided by whether the distance of the little body when at perigee would be less than the radius of the earth. If this were so, then of course the body would strike the earth at its first revolution. If, however, the circumstances under which the body was projected were such that its movement did not bring it into collision with the earth at the first approach, then it would remain as a permanent satellite to our globe. It could never then fall to the earth except under the possible action of such disturbing forces as are generated by the attraction of the sun or the moon. No doubt it is conceivable that under the influence of these disturbing forces it might happen, in extremely rare instances, that a missile, after wandering for ages round the earth in an elliptic orbit, should at last be so deflected from its original path as to strike against the earth's surface. But a little attention will show that such an occurrence must be of a rarity so extraordinary, that it may be dismissed from consideration as the probable source of meteorites, at all events when a more rational explanation is at hand.

The question as to the lunar origin of meteorites has thus become narrowed down to a simple issue. Are there at present any active volcanoes on our satellite? If there are, then it might be quite conceivable that

the meteorites which now arrive here left the moon some few days previously. If, however, the moon's volcanoes are all extinct, it is then excessively unlikely that any of the meteorites that are now falling could have been derived from this source. The lunar origin of the meteorites must therefore be rejected, if it can be shown that the lunar volcanoes are all now extinct.

At the time when it was believed that the moon might be the source of meteorites, there was thought to be good reason for the supposition that some of the lunar volcanoes retained their igneous energy. Modern research has, however, demonstrated that the lunar volcanoes are absolutely silent and ineffective. No doubt some slight indications of change have been detected, in certain details, on the lunar surface, but I do not think that, even if we admit every case of change which has been alleged by recent observers, it could be contended that any one of the lunar volcanoes now possesses the necessary activity. We are, therefore, forced to discard the lunar theory of meteorites altogether, for the simple reason that if the moon ever did project meteoritic masses, they must have tumbled down on the earth ages ago, at the time when the lunar volcanoes were still active. We do not, therefore, look for any lunar explanation of the meteorites which fall down here in these modern days, when the volcanoes in our satellite have become extinct. Let us, therefore, go further a-field, and search for the possible origins of these bodies in the volcanoes of other worlds.

It will be convenient, at this point, to lay down the principle by which we shall be guided in determining the critical velocity which would be necessary to project bodies away from any particular globe. It can be demon-

strated by mathematical principles, on which it is not necessary now to enter, that the critical velocity for each globe will be directly proportional to the square root of its mass, and inversely proportional to the square root of its radius. It can hence be easily shown that, supposing a number of globes are all made from the same materials, but are of different sizes, the critical velocity with which a body will have to be projected upwards varies simply as the radius of the globe. Of course, the condition supposed does not apply exactly to the various heavenly bodies, and, consequently, it would not be correct to assume that the law is quite so simple as that here stated. But for the purpose of our illustration we may so regard it, and this being so, let us consider a globe which has a diameter of 650 miles.

Such a body would be large enough for one of the greater of the minor planets, as large, perhaps, as the planet Ceres. As this is about one-twelfth part of the diameter of the earth it will follow, from the principle we have already laid down, that the speed with which a missile would have to be projected from Ceres, in order to carry it away from that globe altogether, would have to be one-twelfth of that which would be necessary to carry it away from the earth. As already stated, this is a speed quite comparable with that attained by our modern artillery; it therefore follows that if Ceres were 650 miles in diameter, and it must be of dimensions not very greatly differing from this amount, one of our great cannons, pointed vertically on this particular globe, would discharge its missile so that it would not return. It might therefore seem that by locating the volcano on one of the minor planets, a way is offered of getting out of the great difficulty, with regard

to the tremendous volcanic power necessary to impart the acquired velocity to the missile. Quite a moderate volcano placed on such a globe would undoubtedly shoot bodies upwards that would not return. But here, again, we have to remember that before such a missile could descend to our earth as a meteorite, it is necessary for the circumstances of projection to be such that the body shall take a direction which will ultimately cause it to strike the earth. The conditions that this implies are of very great importance. It will be necessary to consider them.

If a meteorite projected from a volcano on Ceres is ever to strike the earth it must, it need hardly be said, pass through that narrow strip in the ecliptic, some eight thousand miles wide, which the earth traces out in its annual movements. I say narrow strip, for although it may seem that eight thousand miles is a considerable width, it must be remembered that on the scale on which the orbit has to be drawn the width named would only correspond to an extremely fine line. The projectile from the planet, as it quits the neighbourhood of the parent globe, becomes appreciably affected by the attraction of the sun, and as its distance from the planet increases, the attraction of that planet dwindles to evanescence, while the attraction of the sun becomes the predominating influence by which the movement is guided. The projectile accordingly pursues a track in accordance with the known laws of planetary motion. We are therefore to think of the little body as revolving around the sun in the same manner as the planet itself revolves, only possibly with an orbit considerably more eccentric, and inclined at a much larger angle to the ecliptic than that at which orbits are generally placed. If, therefore,

this little body is to fall on our earth as a meteorite, it is obviously essential that its orbit shall cross the track followed by the earth. Unless this condition is fulfilled the potential meteorite may pass near the earth on one side or the other, but would not fall down thereon, and we should know nothing about it. We have, therefore, to consider the conditions under which the orbit of the missile shall possess the very fundamental character of crossing the earth's track.

It can be demonstrated by mathematical calculation, as to which there can be no uncertainty, that it would be impossible for a missile projected from the planet Ceres to cross the present track of our earth around the sun, unless at the instant of projection the missile had a velocity, of which the component perpendicular to the radius of Ceres' orbit was about eight miles a second. The actual velocity with which the little body would start on its journey depends partly on the speed with which it was projected, and partly on the speed which the planet has in its orbit. In fact, as a mathematician would say, the velocity with which the object was animated would be the resultant between the velocity of the planet and the velocity imparted by the projecting agent. The velocity with which Ceres moves round in its path is determined by Kepler's law, and it can easily be shown to be about eleven miles a second. It therefore follows from this circumstance alone that the missile will have a speed of eleven miles a second perpendicular to the radius of the orbit of Ceres. The velocity which it receives from the projective force must be compounded with that which it derives from the movement of the planet. We have already seen that it would be utterly impossible for the

meteorite to reach the earth if the component of its

[........ $5\frac{7}{8}$ inches ....]

Fig. 39.—The Middlesborough Meteorite, showing corrugations of melted matter. March 14, 1881.

velocity in the direction of the planet's movement differed from eight miles per second. This consideration shows

that the volcano on Ceres would have to be possessed of very considerable power, quite independently of whatever projective force might be necessary for the mere purpose of conveying the missile clear from the planet.

Let us suppose the most favourable case possible. In other words, let us try to conceive the circumstances under which, with the least expenditure of power, a projectile might be launched from Ceres under such conditions that it should cross the earth's track. It is obvious that this most favourable condition would be presented in the case of a volcano which was so placed on the planet as to lie exactly on the opposite side of the little globe, from that point which was foremost in its motion. For what the volcano has now to do is to abate the velocity which the missile possesses in virtue of the planetary movement, which it possesses in common with every other part of its globe. It follows that the velocity which must be imparted by the explosive power of the volcano, has to be at least three miles a second. For as the little object has a velocity of eleven miles a second in the direction perpendicular to the radius of the planet's movement, it is necessary to reduce this by three miles a second, in order to bring the actual velocity of the planet to the eight miles a second, which we have already stated to be an indispensable requirement if the object is to arrive at the earth as a meteorite. It can be easily shown that a volcano which happened to lie in any other situation than that just mentioned would have to impart an initial speed of more than three miles a second, if it were to reduce the velocity that the meteorite acquires from the planet down to the amount under which alone it would be possible for it to fall on the earth. It thus

appears from the consideration of the orbit of Ceres, and of the orbit of the earth, that a velocity of three miles a second would be demanded by dynamical considerations quite independently of whatever additional speed the missile should receive, in order to carry it free from the attraction of the globe on which the projective agent was situated. No doubt, as Ceres is small, this last might be, as we have said above, a velocity of moderate dimensions, attainable, in all probability, by ordinary artillery. But the velocity which has to be imparted on the other account is so considerable that no matter how small the mass of Ceres may be, a volcano of a projective power of at least three miles a second would be demanded. We thus see that there is no alleviation of the difficulty gained by locating the volcano on one of the minor planets.

Quite independently of this there is a line of reasoning which demonstrates that in all probability meteorites could not have come from any planet situated where Ceres is. It must not be forgotten that the track which the earth pursues in its annual progress round the sun is only an extremely fine line when viewed from the distance of Ceres. This consideration shows that it is only under exceptional circumstances that a meteorite projected from Ceres, even if it had speed enough, should ever tumble on our globe. The question is one in the theory of probabilities. In another part of this volume I have illustrated the importance which the theory of probabilities has for the astronomer. To the cases which are there given I may now add that which is connected with our present argument.

Let us imagine that Ceres was covered with volcanoes; suppose that these volcanoes were from time to time

projecting clouds of missiles with sufficient vehemence to set them finally free from the globe from which they spring. It is not easy to state the question simply, but we must make the attempt. I shall suppose that the speed which the missile receives from the volcano is compounded with that derived from the orbital movement of the planet. We have already seen that if this total speed is less than eight miles a second, then no matter what the direction of the movement of the projectile may be, it must fall short of the earth's track, and can therefore never possibly reach our globe. If, on the other hand, the volcano on Ceres were so powerful that the speed it imparted, when combined with that which the missile derives from the orbital movement, exceeded sixteen miles a second, then the path in which the body starts on its voyage through space would take the form of a hyperbola. In this case, although the missile might cross the earth's track once, it would never do so again, for the attraction of the sun would not be sufficient to recall it. Should the total speed of projection lie between eight miles and sixteen miles a second, then the orbit would be elliptical, and the body would move round and round with the same regularity as a planet. But among all the different possible orbits of this kind comparatively few will actually intersect the earth's track.

To take an illustration, let us suppose the case of those missiles which start with a total speed intermediate between the two extremes we have just named. Let us imagine that they have a velocity of twelve miles per second; it can be demonstrated that projectiles launched forth at the speed just named, but in all directions, will assume all sorts of orbits, and of these

orbits only one out of a very large number can intersect the track of the earth even when due allowance has been made for the effect of perturbation. It therefore follows that out of all the missiles projected from Ceres, only very few could be expected to reach the earth, even after the lapse of an indefinitely great time.

This examination of the conditions under which bodies projected from Ceres could fall to the earth as meteorites, has shown that such a source for these bodies is highly improbable. In the first place, it has been demonstrated that the immediate object sought to be gained by locating the volcano on a small planet would not be realised, for a very high velocity would be necessary on account of the circumstances of the situations of the orbits in the solar system. We should, therefore, in any case need volcanoes with tremendous power even if placed on so small a globe as Ceres. It is further shown, that even if this highly improbable condition could be fulfilled, the volcanoes on Ceres would be so badly adjusted for the work to be done that they would miss a large number of shots for every one that was successful. These improbabilities are so great that we are forced to reject the hypothesis which implies them, especially when, as we shall see in the next chapter, we can point out a locality for the volcanoes to which no such improbability attaches. I need not go into details with regard to the other planets. Setting aside all other objections the large ones would require tremendous volcanoes to drive the missiles free from the attraction of their globes; and there is besides the further circumstance, that there will be in every case an enormous preponderance of missiles which can never pass through the earth's track, over those which may happen to do so.

But before I come to discuss the real source of these bodies it may be well to consider the possibility that meteorites should have been projected from bodies in space which do not belong to the solar system. This is indeed a favourite notion with some, but here again as elsewhere through astronomy the laws of probability afford a reliable guide.

Let us briefly consider the conditions under which a meteorite projected from some volcano located in the stellar spaces would actually pass through the earth's track. No doubt there are scores of millions of stars, and though we cannot see them, there are in all probability thousands of millions of dark globes which, in so far as their non-luminous character is concerned, clearly resemble the earth. It is not improbable that thousands of these globes, or millions of them, may have volcanoes quite as potent, or far more potent than any volcanoes which have ever come within our experience. But even if there were millions of volcanoes or bodies in the stellar space, and even if those volcanoes were powerful enough to discharge missiles which would soar free from their parent globes, the probabilities against the arrival of any such objects on this earth are indeed stupendous. I find it wholly impossible to believe that such can have been the source of meteorites, at all events so long as I can discover another source to which so great a degree of improbability does not attach.

For suppose that a volcano were located on some body lying at the distance of the nearest fixed star, which is believed to be Alpha Centauri. An observer placed in that remote locality and viewing the solar system from the awful distance which intervenes, would see the earth

describe a little circle, about a second and a half in diameter. This is extremely small, it is about as large as a penny piece would look if placed three miles from the observer. The number of such circles, whose collective areas would be required to cover the sky, would be about 500,000,000,000. Now what would be the chance that a rifle bullet, supposing it could carry far enough, directed perfectly at random, would strike a bull's eye the size of a penny piece at a distance of three miles?

It is obvious that the solution to this is to be found in the following manner. Suppose a sphere to be constructed with a radius of three miles, and that the whole inside of this sphere be divided into a mosaic each with an area as large as a penny piece; then as each one of those pieces would be just as likely as any other to be struck by a bullet discharged absolutely at random, the improbability that any particular piece would be hit will be expressed by unity divided by their entire number. A volcano placed at the distance of Alpha Centauri, and discharging missiles quite at random, could only hit that ring which represents the earth's track in one shot out of every five hundred thousand millions. But even these figures do not express the improbability that a meteorite should arise in this manner. For if the missile happened to pass through the interior of the earth's orbit, it would not fall on the earth any more than if it had passed outside the orbit altogether. It is, as we have already explained, an indispensable condition that the body should pierce that particular zone eight thousand miles in width which marks the track of the earth in the ecliptic. As the area of this ring is not the five-thousandth part of the whole area of its orbit, it follows that to pierce the ring

by any one missile a number must on the average be projected, which is five thousand times as great as that already named.

We thus see that the improbability that a body shot from a volcano situated on a globe at the same distance as Alpha Centauri, should ever fall on the earth as a meteorite, must be twenty-five hundred of millions of millions to one. Surely this presents in a very forcible light the extreme improbability that meteorites should have been derived in the way this doctrine suggests. Of course if the volcano were so much further off as to have a distance comparable with that of most of the stars whose distance is known, then the improbability would be still more enhanced. But taking the figures as they stand, it would appear that even if there were at least two thousand million volcanoes, launching forth missiles into space, not more than one out of every million bodies thus projected could ever cross the earth's track, and thus conceivably reach the earth as a meteorite.

It will also be observed that in this calculation I have regarded the earth's track as placed squarely to the line of fire. If it were more or less edgewise, as would of course generally be the case, then the length of the projected track would be correspondingly reduced, and the improbability would be correspondingly increased. From all these considerations I have come to the conclusion that we may reject any hypothesis which would ask us to derive the meteorites from volcanic sources in the stellar spaces. The actual source from which they seem to have come will be considered in the next chapter.

CHAPTER XIV.

THE ORIGIN OF METEORITES.

HAVING thus given grounds for believing that meteorites have come neither from the comets nor from the moon, neither from the planets great nor the planets small, nor yet from the stars or the other orbs which may revolve at stellar distances, I have now to explain the source from which, according to evidence, the meteorites do seem to have been derived. I am, however, quite conscious that the question is by no means free from difficulty. The descent of a meteorite, whatever be its source, is one of the most astonishing facts in nature. The improbability that such an occurrence should take place is indeed great. But we know that it does take place, and the only course to follow in the search for the origin of these bodies is to bring under review all conceivable sources, and then to adopt that one which on the whole appears to have most in its favour.

It seems perfectly demonstrable that one suggested source of meteorites is much less improbable than any of those which have been discussed in the last chapter. We are to advocate a prosaic solution of the origin of these mysterious wanderers which reach us from the

sky by affirming that they have originally been fragments torn from the earth. But that such is indeed the case I shall endeavour to demonstrate. I have no doubt to some extent discussed this matter in previous writings. I have found, however, that the force of the argument is often imperfectly understood, and as the evidence that can be adduced seems to be strengthening in proportion as our knowledge of these bodies accumulates, I have thought it desirable to take this opportunity of treating the question fully with the help of additional information which recent investigations have placed at our disposal.

The notion that terrestrial volcanoes should have been the agent by which meteorites have been driven off into space seems at first to be refuted by the circumstance that we have no volcanoes which possess at present anything like the intensity of explosive energy which would be required. There is, I believe, sufficient testimony to prove that certain bombs projected from Krakatoa in 1883 fell to earth at a distance of fifty miles from their source. It is easy to demonstrate that a body which performed a flight so tremendous must have been expelled from the crater with an initial velocity considerably in excess of any speed that has been attained by a missile in artillery practice. This statement offers a remarkable illustration of volcanic energy. To appreciate it fully we should reflect on the dimensions of a volcanic crater, and on the free opportunity it affords for the escape of the explosive gases, in contrast with the conditions found in the discharge of a missile from a cannon, where all the force of the explosion can be concentrated on the projectile.

It must, however, be admitted that there is no observation, so far as I know, of any volcanic explosion in which

a velocity has been imparted to the fragments discharged exceeding twice that which can be produced by artillery. And yet, as we have shown, we should require a velocity many times as great as that which our mightiest cannons can generate, if the missile so discharged were to be carried free from the earth altogether. No doubt the inadequacy of our present volcanoes is a very great difficulty in the acceptance of the terrestrial theory of meteorites. But the improbability that meteorites should have so sprung appears to me to be very much less than the improbabilities which accompany the supposition that the origin of these bodies can be explained in any of the other ways which have been proposed. It seems therefore necessary to submit this notion of the terrestrial origin to an extremely close scrutiny, and see how far the objections that may be urged against it can be overcome.

I am quite aware that able geologists have maintained that there is no stratigraphical evidence to show that volcanoes of early geological times were more potent than those of the present day. But in so far as such evidence may be wanting I think we can only attribute it to that imperfection of the geological record which has been so often invoked to account for the absence of direct testimony in support of conclusions which there are good theoretical reasons for entertaining. It would seem that the present case is one in which the absence of direct testimony need not greatly concern us. The laws of cooling are not to be impugned, and those laws of cooling declare with unfaltering logic that the thermal conditions to which volcanoes owe their origin must have been such as to make volcanoes far more potent in primeval times while the earth was still young than they are at present.

Whether there be geological evidence bearing on the question, or whether such evidence be wanting, I do not think that the result of the laws of heat will be disputed by any one who is aware of the physical necessity which those laws impose. We can indeed cite analogical evidence from other bodies in the universe in support of our contention as to the superior efficiency of the early terrestrial volcanoes over their degenerate successors of the present day. I have already had occasion to refer to the striking if well-known fact that the lunar volcanoes are now quite extinct. The exhaustion of their primitive power is due to the circumstance that the moon is small enough to have cooled rapidly and thus to have lost almost all its internal heat. It therefore no longer retains the energy necessary for the production of volcanic outbreaks.

Here we have an instance which proves that the uniformitarian hypothesis is not satisfactory so far as lunar geology is concerned. If there were a geologist at present on the moon he would be constrained to admit that his globe was once the seat of volcanic activity of the most widespread character, and of a vehemence and potency the like of which could not be paralleled on any known globe. But while the evidence of this fact was all round him, he would be constrained to admit that his volcanoes had now lapsed into a state of permanent quiescence. So far from uniformity in volcanic matters being the characteristic of lunar geology, we find the volcanic activity of that globe gradually declining through a succession of ages until at last the present stage of absolute inertness has been reached. Uniformitarianism could not be the creed of any lunar geologist;

nor can there be a reasonable doubt that in volcanic matters our earth is progressing along a like course to that which the moon has taken.

Some of our volcanoes still retain activity, because our globe is so large that it has not yet parted with all its initial fiery vigour, but day by day the internal heat is being dissipated. We are tending from a period when the volcanoes were perhaps as important a feature on this earth as they once were on the moon; we are tending towards a period when the volcanoes on this earth shall have become as silent and as extinct as are those craters on the moon, which make our satellite so interesting a telescopic picture. The analogy of the moon will at least justify my contention that the activity of the volcanoes now on our earth is not to be regarded as adequately representing the much more terrific vehemence of the internal heat which must have devastated our globe in anterior stages of its history.

We have next to consider how the volcanic energy of these early fiery mountains on the earth can have produced sufficient explosive power to project missiles into space with a speed at least as great as that critical value to which I have so often referred. It is unfortunate that there is no globe in our solar system which can be seen to be passing at present through the same phase as that through which our earth must have passed in the early times referred to. It would have been so very instructive to have made a telescopic examination of some other world of about the same size as the earth and in about the same stage in its evolution as this globe was before the deposition of the sedimentary rocks. But there is no such globe. Jupiter does not present the conditions, for we are not

able to see its surface through the encompassing clouds. Mars will not answer our purpose, for it seems to have long passed the stage in which volcanic energy was a prominent manifestation on its surface. We cannot examine the details on Venus sufficiently to know how far it might supply the information we seek, and the same may be said of the other planets. They give us but little aid.

There is, indeed, only one globe in the system which

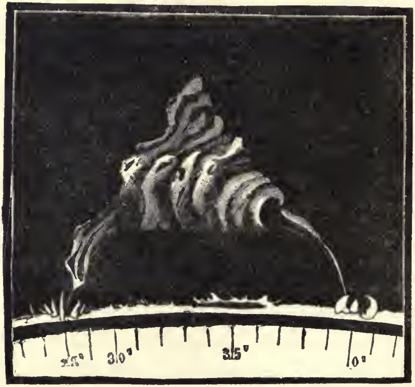

Fig. 40.—Solar Protuberance. April 6, 1892.

can be cited, and that is the sun. Of course, there is but very slight resemblance between the present condition of the sun and the condition of our earth at the early period referred to. The sun is so highly heated that it probably does not contain any solid matter whatever, and therefore is very unlike what our earth must have been at the time of which we speak when its exterior parts had already become solid, if not actually cold. But

there is one matter as to which the sun will render

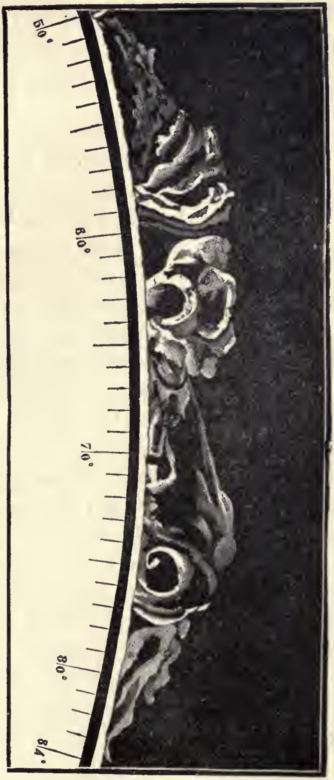

Fig. 41.—Solar Protuberance. April 15, 1892.

valuable evidence pertinent to the question at issue. Remember that our main difficulty relates to the greatness

of the velocity with which missiles must have been expelled by explosions that have taken place in the interior of the globe. We find a difficulty in believing that the nascent earth could have launched projectiles at the speed of seven miles a second. Let us then see what the sun can do.

It can be shown from observations of the solar prominences that vast masses of vapour are frequently expelled from the interior of the sun by convulsive throes with a speed of three hundred, four hundred, and sometimes nearly a thousand miles a second. Let us ponder on these facts. They are indeed most astounding, and would hardly be credible were they not supported by ample evidence of unimpeachable authenticity. The spectroscope enables the observer actually to witness the ascent of these solar prominences, notwithstanding that they are at a greater distance than ninety millions of miles. Careful measurements demonstrate the startling results which I have already stated. Of course, the present fervour and energy of the sun wholly surpasses that which could have been possessed by the earth at the phase of its history which we are considering. But remember that all we want is an explosive power in the terrestrial volcanic gases sufficient to impart a velocity of seven miles a second, nor need this be denied when we have shown that the actual velocities produced by outbreaks in the sun at this very moment are more than one hundred times as great as that modest allowance for which we ask.

Those who ponder on these facts will, I think, come to the conclusion that, though from our ordinary points of view a speed of seven miles a second seems too monstrous to be

admitted, yet that the cosmical forces available at the time of the consolidation of the earth would have been sufficient for the purpose. It must also be remembered that we have no reason to believe there is any substantial difference between the materials in the sun and the materials of which the earth is composed. The gases by which explosions in the sun generate velocities of many hundreds of miles a second, are only the same gases as those which are found on the earth. It therefore appears that by the study of solar phenomena the objection to the terrestrial theory of meteorites on account of the great initial velocity required may be said to have been considerably abated, if not entirely overcome.

There is another objection which has been frequently urged against the hypothesis of the terrestrial origin of meteorites. We have been reminded of the existence of the earth's atmosphere, and attention has been called to the fact that a missile discharged from the lower strata of that atmosphere with an enormous speed would find its velocity greatly reduced by the resistance of the medium through which it had to cleave its way. It has thus been maintained that when the body reached the outer layers of the atmosphere it could not have retained sufficient speed to carry it away from the earth. I freely admit that this is a difficulty. Artillerists know well that the resistance of the air robs their projectiles of energy and greatly reduces the range and the effectiveness of their weapons. Nor am I surprised that this difficulty may have sometimes appeared to be a very formidable one when urged against the doctrine of the terrestrial origin of meteorites. There are, however, certain considerations which will, I think, show that the objections founded on

the resistance offered by the air to the passage of projectiles may perhaps be over-estimated.

In the first place, it must be remembered that the analogy of artillery is hardly to the purpose, for the trajectory of the missile, if not quite horizontal, has at all events no great inclination. The consequence is, that the flight of the body is confined to the lower and denser strata of the atmosphere, where of course the resistance of the air operates much more vigorously. A cannon ball fired vertically upwards would meet with much less obstruction to its upward passage after it had passed through the lower portions of the atmosphere, and the amount of that obstruction would be diminished with every increase in the body's altitude. It is therefore plain that if a volcano were projecting missiles in a vertical direction, as the terrestrial theory of meteorites supposes, the resistance of the air would be much less effective than if the missile were projected in any direction largely inclined to the vertical.

There is also another consideration. It is well known that on the top of a mountain the air is less dense than at the foot. Suppose, therefore, the volcanic discharge takes place from the summit of a lofty mountain peak, the missile will then start on its vertical journey exposed to a resistance not very considerable at the commencement and lessening as each additional foot of elevation is attained. Now there is good reason to believe that in those primeval periods, when the meteorites were launched from the earth, the mountains were more elevated than they are at present. Here, again, I fear I shall have to reckon with the uniformitarian geologist. He will probably dispute this contention, and if he does so I confess that I know no direct facts which I can urge in support of my belief

But my belief is founded on a reason which I hold to be so secure that, until some facts can be brought forward which demonstrate directly that these primeval mountains were not more lofty than those which we now have, I shall continue to think that our present mountains are but degenerate types of the mountains of antiquity.

The reason is a very simple one. The altitude of mountains depends primarily on the resultant of antagonistic agencies. There are the disturbances from below which push the mountains up, for were our earth solid and cold throughout its interior no fresh upheavals could arise. On the other hand, the disintegrating tendencies of air and water tend to reduce the elevations of the mountain summits. In the early days to which we refer the subterranean agents by which upheavals were produced were far more vehement than they are at present. I do not see any reason to think that there has been any corresponding large difference between the efficiency of the disintegrating energies then and their efficiency now. I therefore see no reason to doubt that in days so remote as those of which we are speaking the mountains may under certain circumstances have had greater elevations than any mountains of which we know. A volcano which has its vent in the summit of one of these exceptionally lofty mountains would discharge its missiles vertically, under conditions where the resistance of the atmosphere would be reduced to a mere fraction of that resistance which the artillerist so greatly abhors.

There are also other considerations which may be brought forward in relation to the question of atmospheric resistance. In a vast volcanic outbreak a prodigious volume of gases accompanies the more solid materials

at least in the initial stages of their flight. The whole is projected aloft in such a way that the missiles moving upwards are surrounded by gases or vapours moving perhaps just as fast. In such a case the atmospheric resistance, so far as the missile is concerned, is reduced to a mere nothing.

In considering the objections arising from the resistance of the air to the terrestrial theory of meteorites, observations on the sun are very instructive. It is known that our great luminary is surrounded by a prodigious atmosphere which would expose to missiles projected through it a resistance enormously in excess of that which any bodies projected from the surface of the earth would encounter in their passage through our atmosphere. And yet we find that in some of the great solar eruptions in which metallic vapours are projected outwards, a prodigious elevation is frequently reached by these gaseous masses, notwithstanding the resistance of the medium through which they have to force their way. Indeed, observations have been made which seem to show that the velocities thus attained are frequently sufficient to force the emitted gaseous volumes completely through the encompassing atmospheric envelope, and even then leave them at the exterior of the solar atmosphere with sufficient speed to carry them entirely away from the sun on a voyage through space. Here, then, we see that the explosive forces on one body of our system are amply tufficient, not alone to overcome the retardation due to she resistance of its atmosphere, but to allow the missile to depart from its precincts with a speed which is nearly a hundred times as great as that which would be required to expel a projectile from our earth.

After attentive consideration of the various points which have now been discussed, it will, I think, be admitted that the great initial velocity which the terrestrial theory of meteorites demands can no longer be deemed an insuperable objection to its acceptance. We need not ask that the necessary violence shall have been manifested very often in comparison with the total number of volcanic outbreaks. All we demand is, that during the millions of years in which the crust of the earth has been consolidating there shall have been occasionally outbreaks of sufficient vehemence to have discharged clouds of fragments with such energy that they shall leave the earth, or rather the earth's atmosphere, with a velocity of not less than six miles a second. If this be granted, then the explanation of the origin of the meteorites is complete.

Each little particle after taking leave of the earth to commence its voyage through space, will be acted upon by the attraction of the sun. To a certain extent, of course, the attraction of the earth must also affect the movement of the body, but after a time it will generally happen that the attraction of the sun will become of paramount importance in the control of the movement. This will be obvious when it is remembered that the sun is more than three hundred thousand times as massive as the earth, and that therefore at equal distances the potency of the sun's attraction when compared with that of the earth is to be expressed by the same number. We know that the attraction varies inversely as the square of the distance, so that by the time the missile has reached a point which is about the five-hundredth part of the present distance between the earth and the sun, the attraction of these two bodies on the missile will be equal. In fact, long ere the little

object had reached a distance which is as great as that between the moon and the earth, the sun's attraction would have surpassed the attraction of the earth.

Assuming that the body moved in a straight line with a speed of seven miles a second, it is easy to show that after eight or nine hours it would have passed more under the influence of the sun than it was under that of the globe from which it had taken its rise. It is, therefore, obvious that ere long the object would be so far affiliated to the sun that it would have practically renounced all connection with the earth, which would now mainly operate as a perturbing agent in its movement. The laws of dynamics assure us that under these circumstances the missile will continue to move in what is practically an orbit around the sun. The size of this orbit will depend upon the particular circumstances of the case.

Let us suppose that the initial velocity does not exceed a certain definite amount. Then it is plain that the figure in which the object is constrained to move must be an ellipse, the motion being performed in accordance with Kepler's laws, around the sun placed in the focus. The plane in which the little body moves as well as the time which the journey requires depends on the initial conditions as to speed and direction under which the projection took place. It is all-important for our present purpose to know that the orbit must cross the point from which the meteorite originally took its departure. Perhaps it would be more correct to say that it must pass in the vicinity of that point, for the attraction of the earth will no doubt slightly modify the initial circumstances of the movement, and indeed there are other perturbations which ought not to be overlooked. We

may, however, conclude that in its voyages through space the meteorite pursues a track which generally or frequently crosses the earth's track. This conclusion is a most essential point, and in it lies the fundamental argument in favour of the terrestrial origin of meteorites.

Have we not seen the improbability that a meteorite projected from a minor planet, such as Ceres, shall fulfil the necessary condition of crossing the earth's track, for there are so many chances against it? Have we not also seen that a meteorite projected from a globe lying in the stellar regions would almost certainly fail even to cross the orbit of our earth, inasmuch as the probability of its doing so would be only one against many thousands of millions? When, however, we locate the volcanoes which are presumed to be the origin of meteorites on the earth, these improbabilities disappear altogether. Instead of the chances against the orbits of these bodies crossing the earth's track being millions to one, or thousands to one, the fact that they must generally or frequently do so has become a demonstrable certainty. Indeed, they would always do so were it not possible in certain cases for the existence of disturbances, such as would be produced by the passage of a meteorite near the moon, to derange the path so far as to warp the little body into a direction which passed slightly within or slightly without the earth's track, instead of directly through it. There is also the effect of planetary perturbation both on the orbit of the meteorite and on that of the earth to be borne in mind. But the probabilities of the situation have been entirely transformed when we consider the terrestrial theory of meteorites. The probabilities are now that the missile does fulfil the necessary condition of crossing the earth's

track, while on the other theories the probabilities ranged themselves against this fundamental requirement. Every missile projected from a mighty primeval volcano on this earth with the requisite speed will be freely revolving around the sun in an elliptic track, and frequently crossing the earth's path.

We have learned from Tschermak the fundamental fact that in all probability meteorites owe their origin to some volcanic source. Adopting this as a fundamental position, I have given the astronomical grounds which appear to demonstrate conclusively that these volcanoes must have been on our own earth. I am, however, at once met by the objection, that the mineral substances found in meteorites are not those which mineralogists generally speaking recognise as the products of terrestrial volcanoes at the present day. I fully admit this, indeed it is well known that in many cases a meteorite can be shown by chemical analysis alone to be one of those bodies which has fallen from the sky. Not only the actual minerals which are present, but the form in which they are associated, are generally speaking so characteristic that the meteoritic character of a stone can often be unhesitatingly pronounced upon even though the fact of its fall is entirely unknown. This is a very great difficulty, and I should be inclined to regard it as an insuperable one were it not for a particular circumstance on which I must now dwell.

No doubt it is quite true that the character of a mineral mass will, generally speaking, enable the experienced mineralogist to pronounce decisively whether the object is meteoritic or whether it is not. There is, however, one notable instance in which the ordinary rules for the diagnosis of a meteorite would have certainly proved

fallacious. I allude here to the famous mass of so-called meteoric iron discovered by Nordenskjöld, at Ovifak, on the coast of Greenland. Great masses of metallic iron amounting collectively to many tons were found at the place which we have named. The occurrence of iron in a metallic state is a circumstance so unusual that specimens of this body were examined with particular interest. The iron was found to be mixed with nickel, thus producing the remarkable alloy generally recognised as characteristic of meteorites. This suggested that these Ovifak irons might have tumbled down from the sky, like other masses of iron-nickel alloy which were known to have done so. The conclusion was indeed quite a natural one when it was observed that the metal in question differed in no perceptible respect from that in undoubted meteorites.

Close examination on the actual sea-shore where these masses were found has, however, made it clear that the famous Ovifak irons have come, not from the heavens above, but from the earth beneath. There are a large number of pieces which lie imbedded in basalt, and as the surrounding rock has weathered away the lumps of iron have become exposed. It is also plain from the complete manner in which the irons are incorporated with the once molten lava that it would be absurd to attribute to them a meteoritic origin. Had lumps of iron-nickel alloy been lying simply on the basalt beneath, then of course they might have been concluded to be meteoritic with just the same logic as that by which a celestial origin was attributed to the great iron mass found by Pallas in Siberia. But it is perfectly plain that the Ovifak iron has once been surrounded by *molten* lava, for sections of some specimens exhibit in the most striking

manner the way in which the iron graduates into the adjacent minerals.

A meteoritic origin of these masses would be only possible on a wholly preposterous hypothesis. To account for them on such a theory we should have to suppose that at a time when molten lava was poured forth from a volcano or was welling upwards from the earth's interior to the surface, it so happened that an unparalleled fall of tremendous iron meteorites plumped down from the sky just into that particular spot which the lava occupied. We need not entertain a supposition so widely improbable. It is perfectly plain that the Ovifak irons have not come down from above. We must adopt the other alternative that they have come out with the basalt from the interior of the earth.

This is a conclusion of an extremely instructive character. We may note that it derives confirmation from the discovery of Andrews that metallic iron, though no doubt in minute particles, was a constituent in some specimens of basalt obtained in the north of Ireland. Other circumstances corroborate the notion that iron is extremely abundant in the interior of the earth. The phenomena of terrestrial magnetism indicate that this element must be an important constituent in the earth's interior. The argument derived from the density of the earth also deserves notice. It has been established that the mean density of our globe is upwards of five times that of water. But even the heaviest rocks which lie on the earth's surface have a density scarcely three times that of water. Of course it must be admitted that the materials in the centre of the earth are exposed to prodigious pressure, but yet there is nothing we know as

to the nature of granite, whether solid or molten, which would lead us to believe that any amount of pressure could elevate its density from three to five. The only way of accounting for the high density that the earth exhibits as a whole, is by the assumption that there must be vast metallic masses in the interior. Considering the well-known abundance of iron in the earth's crust it can hardly be doubted that this is the element which gives its high specific gravity to our globe.

Spectroscopic evidence as to the presence of iron elsewhere demonstrates its abundance. Iron exists in profusion in the sun and in many of the stars, so that taking all these matters into account it seems highly probable that this element abounds in the deep interior of our earth. This being granted there is now no difficulty in accounting for the presence of the great Ovifak masses. As the molten lava issued from some volcanic vent it swept forth mighty fragments of that iron whose abundance in the earth's interior is shown on other grounds to be so extremely probable.

There is also one other instance which may be adduced in which specimens of this peculiar iron-nickel alloy have been shown to be of terrestrial origin. There is a remarkable extinct volcano known as Coon Butte in Mexico. It is one of many others which indicate that the country which contains them was at one time intensely agitated by volcanic forces. All the evidences of volcanic activity are around, the congealed streams of lava are still to be seen, as well as the craters from which that lava has emerged. The circumstance in connection with this region which is important for our present purpose is the presence of a multitude of masses of metallic iron which

are strewn around. These irons so closely resemble undoubted meteorites both in appearance and in composition that it was perhaps not unnatural that they should have been deemed to have had a celestial origin, and the fact that these so-called visitors from the sky were strewed in and around the volcanic vents has been commented on as a remarkable coincidence.

With the knowledge obtained from the Ovifak iron, we can at once place what is doubtless the true interpretation on the interesting phenomena at Coon Butte. It is perfectly certain that these iron masses never came down from the sky. We do not doubt their resemblance to meteoritic bodies; in fact that there is such a resemblance is a part of our argument, but what we desire to point out is that they have been simply extruded from the earth itself in the course of a volcanic eruption. Their existence in their present situation forms an independent line of testimony to show that so far from the iron-nickel alloy being only found in genuine meteorites, it is distinctly a terrestrial substance appearing abundantly in certain volcanic localities. I would therefore look at Coon Butte as an illustration on a feeble and degenerate scale of one of the mighty volcanoes of ancient days.

I do not think, indeed suggest, that this particular volcano was one of those from which meteorites have been launched. Probably at the comparatively modern date when Coon Butte itself was active, its energy was of a far less potent description than that which would have been required for driving the missiles free from the earth. But it requires no great effort of the imagination to think of a primeval volcano charged with materials

similar to those which now lie around Coon Butte and testify to its former activity. If it had projected those materials aloft with the necessary speed, they might well have been the meteorites which sometimes fall down here to-day.

In conclusion I will just recapitulate the main points of the argument on which we have been engaged in this chapter and the last. I have started from the doctrine put forward by Tschermak, and not, so far as I know, successfully impugned by any other mineralogist, that meteorites have had a volcanic origin on some large celestial body. I have examined the different globes that might possibly be presumed to be the source of the meteorites. I have shown that though the moon may once have been the parent of certain missiles, it cannot be reasonably held to be the source of the meteorites which now fall, inasmuch as the lunar volcanoes are now extinct, and the ejects of the volcanoes of a past epoch must, if they escaped at all, have fallen long ago or shortly after their ejection. I have then brought under review certain of the planets belonging to our system. I have shown how it would be extremely improbable for any missile projected from one of the smaller of these bodies ever to tumble on the earth. I have also shown that the orbits of the small planet are so situated that a very high velocity of projection would be necessary, in order to convey a missile from a volcano there to a resting-place here. I have shown the extreme unlikelihood that any missile discharged from a volcano, on a globe lying somewhere in the stellar distances, could ever reach this earth. I have shown this improbability to be so great, that even though there might be hundreds of millions of

presumable globes throughout space, we cannot entertain the supposition that any missile starting from one of them could ever fall down here. I have shown that meteorites cannot be reasonably associated with comets, notwithstanding the undoubted alliance which exists between comets and shooting-stars.

In the search for the body which must be credited with the parentage of the meteorite I have come at last to our own earth. It is true that difficulties have been urged against the view that meteorites have been derived from the globe on which we live. I have endeavoured to remove these difficulties, and I believe I have shown that they are, at all events, far less considerable than those which are experienced when we endeavour to attribute the meteorites to any other source that has been alleged. The objections that are felt to the view of the terrestrial origin of the objects are threefold. They are, firstly, the excessive initial velocity which would be required; secondly, the interference which the resistance of the air exposes to the passage of a meteorite from this globe to outer space; and, thirdly, the circumstance that meteorites do not resemble the more familiar substances recognised as belonging to terrestrial volcanoes. I do not think I am over sanguine in the belief that, serious as these difficulties may seem, they can be overcome.

In the first place, with regard to the initial velocity, I have pointed out that velocities, even one hundred times as great as that which we require, are at this moment occasionally imparted by explosive outbursts of the sun. Why, then, may not eruptions possessing one-hundredth part of the power of those which we see in the sun have taken place in our earth in primeval days,

shortly after it had attained a solid exterior, but while its internal fervour was more vigorous than it is at present? I have pointed out, that though a great column of gases and vapours, projected vertically from a volcano, may be exposed to tremendous atmospheric resistance at its exterior, it may yet contain missiles in its interior whose movements are but little interfered with. In illustration of this I cited the well-known fact, that notwithstanding the resistance of the solar atmosphere, metallic vapours are often projected through it, with prodigious velocity, to an altitude of scores of thousands of miles. Lastly, I have dealt with the argument that may be derived from the fact that the minerals at present most abundant on our earth are not those which come down in meteorites. It has, however, been demonstrated, both at Ovifak and at Coon Butte, that the iron-nickel alloy, which is above all other substances most characteristic of meteoritic masses, is a terrestrial mineral. It has been found in the two places I have named, under such circumstances as to prove that it must have been extruded from the earth's interior. It therefore seems that there can be no doctrine, with regard to the source of meteorites, which has the same probability in its favour as that which assigns to them an origin in volcanoes in a primeval condition of the earth.

CHAPTER XV.

THE CONSTITUTION OF GASES.

OF late years the attention of those who study the mysteries of nature has specially tended in the direction of examining the very smallest particles into which the chemical elements can be divided. It is true that these particles or molecules, as we generally call them, are too minute to be appreciated by our senses; they cannot be detected even by the most potent microscope. Indeed, if we will but think of it, we could hardly expect our nerves to disclose the character of objects so minute as molecules. Like all the rest of matter, nerves are constituted of molecular particles, and the structure of the most sensitive fibre is far too coarse to transmit indications of the special characteristics of the molecules, of which matter in general is composed. The little objects about which we are to write are things which we can never see, which we can never feel severally. They require to be brought together in clustering myriads where individual peculiarities are merged in general properties before they can make any successful appeal to our organs of sense.

How then, it may well be asked, are we able to learn

anything as to the nature of objects which so successfully elude giving any direct testimony appreciable to our senses? Seeing this difficulty, it can hardly be surprising if some should have doubted the accuracy of the results at which philosophers have arrived with regard to the ultimate constitution of matter. But there are methods of discovering truth which are in certain cases capable of more subtle work than the direct indication of our senses. It is these indirect processes which have taught us much regarding that invisible world which lies around. In certain respects we may contrast the subject about which we are now to be engaged with those great themes which more usually occupy the attention of astronomers. In the case of the heavenly bodies the mind is taxed by the effort to conceive distances so tremendous, masses so enormous, and periods so protracted that we often despair of obtaining adequate notions of magnitudes which altogether transcend our ordinary experience. We are now to make an appeal to the imagination in a precisely opposite direction. We are to speak of masses so minute, of distances so short, and of periods so infinitesimal, that it is utterly impossible for us to parallel them by the phenomena with which our senses make us directly acquainted.

At both extremes, however, we employ the same weapons for the study of the phenomena of nature. Mathematical investigation submits to no restriction either in the greatness of the space over which its command extends, or in the minuteness of the portions which must obey its laws. The principles of dynamics are equally applicable whether the periods of time which they contemplate shall be millions of years, as they often

are in astronomy, or the millionth part of a second, as they often are in physics. Let me here endeavour to set forth some of the results at which natural philosophers have arrived with regard to the ultimate constitution of matter.

Take a lump of loaf sugar and crush it in a mortar, each of the fragments is, of course, a particle of sugar still. Let the operation of grinding be carried on until the entire lump has been reduced to powder of the utmost fineness, which any grinding apparatus is capable of effecting. Each of the minute particles is still, nevertheless, a fragment possessing the attributes and properties of sugar. It has the sweetness and the hardness, the solubility and the chemical composition of the original lump. There is a difference in dimensions, but no difference of any other kind. But now let us suppose that we were in possession of some pulverising apparatus which would permit the reduction of the sugar to be carried on to an extent far greater than that which could be obtained by the most perfect grinding-mill known to the mechanic. The sugar might be comminuted by such agency to so great an extent that the little particles into which it had become transformed could only be discerned as the smallest of specks under the most potent of microscopes. We have the best reasons for knowing that even these little specks, which are of such extreme minuteness that the original lump contained many millions of them, are still, neither more nor less than sugar.

Up to the present stage the reduction has not transformed, so to speak, the actual nature of the material submitted to the treatment. Though the particles have been crumbled to such an extent that after any further

diminution they would cease to be visible, even in the microscope, yet we can, at all events, conceive that further disintegration could be carried on. In fact, the very smallest of these grains, only just visible under the microscope, might be crushed into a thousand parts, and still each little part would not yet have lost the attributes which belonged to sugar. We have now arrived at the conception of a magnitude too small to affect any of our senses, no matter how they may be fortified by the aid of instruments. But the trituration may be conceived to be carried on one step further, until, at last, the original lump has been reduced to particles of sugar so small as to admit of no further subdivision without a total transformation in character. This is an extremely important point. It may, in fact, be regarded as one of those cardinal doctrines which it has been the glory of modern science to teach. There was a time when it was believed that the subdivision of a particle of sugar might be carried on indefinitely. We now know that is not the case. We know there is a certain portion so small that it cannot be again divided. I do not mean that this particle is not in itself composed of separate objects, but what I do mean is, that if, when we have an ultimate particle of sugar, it were divided into two parts, as it might be by chemical processes, neither of those two parts would be sugar or anything like sugar. They would each be something which possessed neither the hardness, nor the colour, nor the sweetness, nor, indeed, any of the attributes characteristic of the original material.

This same argument may be applied to every other substance besides that which I have taken as a first illustration. The atmosphere, composed as it is of oxygen

and nitrogen, with traces of other gases, must consist in ultimate analysis of myriads of gaseous molecules. There are molecules of oxygen, and molecules of nitrogen, as well as molecules of the other gases. No doubt the molecule may consist of parts; indeed we know, as a matter of fact, that it does consist of parts; it is therefore conceivable that the molecule could be subdivided, but these parts, whatever other properties they might have, would certainly not possess the characteristics of the original gas to which the molecule belonged.

It is something to have learned so much at all events with regard to the composition of the air we breathe. But we have been able to find out a great deal more, and to discover that the behaviour of these little molecules is of the most extraordinary and vivacious description. That air is eminently mobile is sufficiently obvious; but the air which fans our faces, or the wind which turns our windmills, or even the hurricane which devastates whole towns, gives only a very imperfect idea of the real mobility of air. The hurricane may move, perhaps, at a pace of a hundred miles an hour, but that is a velocity which might almost be described as sluggish in comparison with the molecular movements of the ultimate particles of gases. For our present purpose we may think of the air, not as it blows over the mountain tops, but as it lies in some secluded cavern where all seems absolutely still. No doubt to our ordinary methods of investigation the air and every particle of it may seem in such a case to be absolutely quiescent, but we have ascertained that the apparent quietness is really the result of the want of sufficient acumen in our organs of sense. Had our perceptive powers been endowed with the necessary subtlety, we

should have perceived that the apparent calm had no real existence. On the contrary, it would have directly appeared that the ultimate molecules of the air were in a condition of exuberant liveliness.

No doubt the smallest particle of air which could be directly appreciable by any of our appliances for measurement, seems a homogeneous structure and perfectly quiescent in its several parts. When, however, we conduct the examination into its character by those refined methods of investigation which are now at our disposal, we find that the air is ultimately composed of myriads of separate particles. Each of these little particles, no matter how quiet the air as a whole may seem, is in a state of intensely rapid movement. Picture to yourself incalculable myriads of little objects, each dashing about with a speed as great as that of a rifle bullet, and often indeed far greater. The little particles are so minute that it would take about fifty millions of them, placed side by side, to extend over a single inch. The smallest object which we can discern with a microscope is perhaps one hundred-thousandth of an inch in length. The little gaseous molecule would therefore require to possess a diameter about five hundred times greater than that which it actually has, if it were to be large enough to admit of inspection by the utmost microscopic powers which we could bring to bear upon it. And yet, notwithstanding the fact that these particles are so extremely minute, we are able to reason about their existence, to discover many of their properties, and to ascertain the laws of their action in such a way as to throw light into many obscure places of nature. I do not, indeed, know any doctrine in modern science of a more instruc-

tive character than that which teaches us the composition of gases.

The movements of the gaseous molecules are, however, so wonderful that it is hardly surprising that those who are invited to believe these things should demand satisfactory evidence as to the existence of phenomena, which from the nature of the case seem to lie out of the reach of direct inspection by the senses. The methods by which our knowledge of the constitution of matter has been obtained, is by reasoning from the phenomena which our senses reveal to the more refined and supersensible conceptions of molecular physics. I could not undertake in a work of this description to give any complete account of the evidence by which these remarkable doctrines are sustained. I will therefore only indicate one of the main lines of argument, by which the necessity for the belief of an important part of the molecular doctrine of gases has been satisfactorily demonstrated.

There are no more fundamental properties possessed by a gas than those which are connected with the pressure which it exerts. If included within a closed chamber, a gas presses against the walls of that chamber. In this case there is a notable contrast between the behaviour of solids, of liquids, and of gases. Suppose, for instance, that a cubical box be fitted with a block of iron which fills it exactly. The metal presses, of course, on the bottom of the box, but not on all its sides. If the iron be removed and the cubical chamber be now filled with water, the liquid presses as before on the bottom with its entire weight, but in addition there will be a lateral pressure exerted by the liquid against the sides of the vessel. Even in this case, however, there is in general

no pressure on the upper surface, and to this extent the behaviour of the water resembles that of the iron. Finally, suppose that the box be filled with gas or vapour, this substance will, like the solid or the liquid, press on the bottom of the box, with the entire weight of the gas, it will like the liquid exert a lateral pressure, but it will differ from both the other substances by its manifesting an upward pressure. It will generally, therefore, be necessary to provide a cover for the box, or if there be no cover to expose the upper surface of the gas to pressure in some other way, in order to retain it within the limits of the cube. No doubt under ordinary circumstances we may fill a vessel to the brim with carbonic acid gas, and the gas will remain much in the same manner as if the vessel had been filled with water. But the carbonic acid in such a case is kept down by the pressure of the superincumbent atmosphere. If that pressure were removed the gas would speedily expand and overflow, unless a cover were provided by which it was restrained, and then the gas would exert an outward pressure upon the cover, showing that it had the tendency to expand even though actual expansion was not permitted.

It will be seen that in this behaviour a gas is totally different from a solid or a liquid. No doubt evaporation is generally speaking taking place from the upper surface of a liquid, and the vapour thus produced acts as a gas, so that in this respect some slight qualification of the above statements might be necessary. But nothing can be more obvious than the fact that the upper pressure of the gas indicates some profound difference between its molecular structure and that of a solid or a liquid. We are so accustomed to this

manifestation of gaseous pressure that its extraordinary character is apt to be overlooked on accounts of its familiarity. Let us, therefore, consider the matter carefully, and perhaps the simplest method of doing so will be to think of a vertical cylinder with a piston that fits closely, but can move freely. Beneath this piston gas is supposed to be present, and we may imagine the pressure to be so adjusted that the piston shall be ascending in consequence of the pressure exerted by the gas. The question to be examined relates to the force by which this upward movement of the piston is caused. At first it might be urged that the force is simply due to elasticity, and of course this is true, though it is far from providing the required explanation of the difficulty. Let us look a little closer into the matter and see if we cannot ascertain what may be the physical character of this so-called elasticity. We have seen that gas is composed of an innumerable host of molecules, and therefore it must be in some way owing to the action of the molecules that the piston is compelled to ascend in opposition to gravitation.

It is necessary to believe that what elevates the piston is nothing more or less than the hammering of the little gaseous molecules underneath. If the gas be compressed into half its volume then the distances of the molecules are lessened, and the number of blows that the piston receives in a given time is doubled, so that the force with which the piston is pushed upwards is also doubled. This accounts for that fundamental property of the gas which declares that when the temperature remains unaltered the pressure varies inversely with the volume. The effect of heat in increasing the pressure of a gas can be simi-

larly explained. If the gas beneath the piston be heated the velocities with which the molecules are animated become increased. The energy with which they rap on the piston likewise becomes greater, in other words the effect of heating a given volume of gas is to increase its pressure. We thus see how the well-known properties of gases can be completely accounted for on the supposition that their constitution is precisely that which the molecular theory affirms.

As the gaseous molecules dart about they frequently come into collision. The effect of such a collision or encounter, as it is more properly called, is to deflect each of the molecules from the rectilinear path, which it had previously pursued, and to send it off in a new direction. These collisions take place with such frequency that in gas, at the ordinary temperature and pressure, each molecule experiences them at the rate of millions in a second. The path of each molecule thus consists of the free parts, during which it is practically uninfluenced by other molecules, and the disturbed parts during which it is acted upon by the molecules with which it has been fortuitously brought into collision. The frequency of these encounters depends, among other things, upon tne density of the gas. In gas of the utmost rarity, such as that which is contained in the vessels employed by Mr. Crookes for his radiometers, the free path of the molecule may be as much as a quarter of an inch before it is turned aside in an encounter

The actual parts of the molecule are themselves in a state of active vibration, and the nature of that vibration is often of a highly complex character. Notwithstanding the extreme minuteness of the vibrating particles, we are

able in some degree to learn certain properties of the vibrations with which it pulsates. Let us take, for instance, the element hydrogen and see what can be ascertained with respect to the molecules of that gas and their vibration. For the purpose of the experiment hydrogen in a state of extreme rarification is put into a tube, and a current of electricity from the induction coil is passed through it. The gas begins to glow with luminosity, and when the light is transmitted through the slit of the spectroscope lines characteristic of hydrogen are displayed. The visible lines are now known to be only a small part of the total spectrum of this element. For when the radiation from glowing hydrogen, either as obtained from a terrestrial source or from some of the stars, is photographed after passing through the prism, several lines are indicated that do not consist of light visible to the eye though visible to the peculiar sensibility of the photographic plate.

This system of spectral lines, so characteristic of hydrogen, must arise in some way from the molecules of which we know the gas to be constituted. We shall, therefore, consider how such effects are produced. A bright line, such as one of those of which the hydrogen spectrum is composed, arises from vibrations in the ether of one definite refrangibility. The effect of transmitting light through a prism is to sort out the different rays in accordance with their several refrangibilities. When, therefore, the spectroscope shows that light from incandescent hydrogen resolved into a number of bright lines, it thereby demonstrates that the radiation emitted from the glowing gas consists of just so many rays of the particular refrangibilities to which those lines correspond. But the re-

frangibility of a ray of light depends upon its wave-length. Hence, then, we see that glowing hydrogen emits a number of rays possessing certain definite wave-lengths and no others. The rays of intermediate wave-length are entirely wanting. In this respect, of course, a fundamental contrast is presented between such a spectrum as that we are considering and the spectrum of an incandescent solid in which light of every wave-length between certain limits is manifested. Each wave-length corresponds, of course, to a certain system of undulation in the ether. We are hence assured that the radiation from hydrogen translates itself into a certain system of waves of ether, each with its own particular period.

We must, therefore, expect to find that the hydrogen gas possesses the means of imparting to quiescent ether those particular vibrations that the spectroscope reveals. It will be obvious that the movements of the molecule of hydrogen as a whole are not what will answer the purpose. Such movements are not in the nature of vibrations. It is indeed known that the speed with which a molecule of hydrogen is animated undergoes frequent changes. If the positions of the lines in the spectrum were directly dependent on the velocity of translation of the molecules, the spectrum could not be expected to exhibit the characters which we actually find it to possess. It is, therefore, impossible for the origin of the spectral lines to be attributed to any other source than those internal agitations which each molecule itself possesses. The oscillations of the several parts of the molecule impart vibrations to the surrounding ether which are from thence propagated as radiant light. The vibrations of an elastic body are isochronous, that is to say, per-

formed in equal times, and to this extent the molecule behaves as an elastic body. The pulses which it imparts to the ether just possess the properties requisite for the production of light.

It is, however, necessary to suppose that the vibrations which the molecule communicates to the ether are not solely of one type. Were this the case only one set of waves would be propagated, and there would only be a single line in the spectrum. In the spectrum of hydrogen, however, there are, as we have seen, quite a number of lines. These seem to resemble harmonics of some fundamental note. It would, therefore, appear as if the molecule of hydrogen, in addition to its fundamental vibration in the note which properly belongs to it, possessed a number of subsidiary vibrations which might properly be regarded as harmonics. The molecule is thus acting in much the same way as a musical instrument, which, in addition to the primary note to which it mainly responds, disperses at the same time and in consequence of the same impulse a number of fainter notes which are harmonics of the fundamental one.

It is quite evident that the molecule of hydrogen must be of a much simpler character than the molecule of many other elements. For though the spectrum of hydrogen contains a large number of lines, most of them, if not indeed all, belong to a group of associated vibrations. In the case of other elementary bodies the complexity of the spectrum is such as to make us think that the vibrations of the molecule must be of a very complicated character. Thus in the case of iron, we find that when this element is brought to the gaseous state by heat, the light which it emits has a spectrum containing some thousands of lines.

It may no doubt be true that groups of these correspond to harmonics of a smaller number of fundamental notes, but even with this admission the vibrations of the molecule of iron must necessarily be of a highly elaborate description.

The effect of heating a gas is to make its molecules move more rapidly. When this happens the collisions between the molecules take place with greater vehemence, and the internal agitations of the molecules arising from the shocks of their collision are all the more intense. Hence, when the gas becomes hot enough, the molecules vibrate sufficiently to produce undulations in the ether strong enough for the perception of light.

Much of what has been said with regard to light may be repeated with regard to heat. We know that radiant heat consists of ethereal undulations of the same character as the waves of light. Hence, we see that the heat or the light radiated from a glowing gas is mainly provided at the expense of the energy possessed by the molecules in virtue of their internal oscillations.

One of the most instructive applications of these principles is to afford an explanation of the means by which the sun sustains its heat, of which we have already spoken in a previous chapter. As the great luminary is a mass of glowing gas or vapour, it is of much interest to examine how far the doctrine of the molecular theory of gases can answer the great question of solar physics. It has of course been long known that the sun retained its power of radiating heat for so many ages in virtue of its contraction. Helmholtz had shown that the amount of potential energy due to gravitation in a mass of matter equal to that of the sun, and expanded over a volume as

great as that which he occupies, would completely account for solar radiation. It was only necessary to suppose that this volume of matter contracted in consequence of the mutual attraction of its parts. As it diminished in bulk, the quantity of potential energy would be of course lessened. But as energy could not be lost, that disappearing potential energy must be manifested in some other form. This was accomplished by its transformation into heat, which kept the sun so far supplied as to maintain its radiation unabated for uncounted thousands of years. It was easily demonstrated that a shrinkage in the solar diameter too small to be appreciable by any measurements we could make, would, nevertheless, set free a quantity of heat sufficient to maintain the radiation for a period of two thousand years. The molecular theory of gases stands in a significant relation to this beautiful discovery of the great German philosopher. It is quite clear that the necessary energy is indeed afforded by the contraction, but it is not quite so easy to learn the precise character of the process by which the energy after disappearing from the potential form reappeared as heat. We want as it were to see the mechanism by which this is effected. This it is which the molecular theory of gases enables us to do; we can now follow the entire process of transformation which the energy undergoes.

Gravitation at the surface of the sun is of course very much greater than at the surface of the earth. It is easy to show that if two globes had the same mean density the gravitations at the surface of each would be simply proportional to its radius. As the radius of the sun is 109 times as great as the radius of the earth, it would follow that if the earth and the sun had the same mean density

the gravitation on the large globe would be 109 times as great as that on the earth. It is, however, known that in consequence of the high temperature of the sun its materials are so much more expanded than are those of the earth, that the sun's mean density is only about one-quarter of that of the earth. In consequence of this we see that the gravitation of a body on the sun's surface must be one-fourth of what it would have been if the sun had a mean density equal to that of the earth. It thus appears that the gravitation at the surface of the sun must be about 27 times as great as the gravitation on the earth.

The effect of gravitation on our globe is well known to be able to impart to each body in the course of one second a velocity equal to 32 feet per second. It therefore follows that a body falling at the sun's surface receives in each second an increment of velocity to the extent of 864 feet per second. But the visible parts of the sun are composed of gaseous or vaporous materials. From the molecular spectrum of gases we have been taught to believe that the molecules of which the sun is composed are in incessant motion. Gravitation constantly tends to impart to each molecule an increase of velocity downwards. It is quite true that we cannot expect each molecule should actually acquire an additional velocity of 864 feet each second. Indeed, in our own atmosphere we have an illustration of the absurdity of such a notion.

The gravitation on the earth imparts to every falling body a speed of 32 feet per second. Of course if there were only a single molecule coming in from outside space it would doubtless hurry in towards the earth

with a gradual augmentation of velocity at the rate we have named. Had our atmosphere been originally in a highly diffused state, gravitation would have drawn it in to the stable condition which it at present occupies. The condition of equilibrium in our atmosphere appears to be as follows. At the surface of the earth there is of course an unyielding surface so far as the air is concerned. We may regard the atmosphere as divided into a number of concentric shells around the solid sphere. Each shell has a density less than that of the shell above it, and greater than that of the shell below it. The upward pressure from the lower shell compensates for the effect of gravitation on the shell above, and thus the equilibrium is sustained. But in the case of the sun there seems to be no solid shell possible. The consequence is that there must be a general shrinkage of the entire mass; this being so the molecules on the whole get nearer to the sun's centre, and consequently, in virtue of the attractive power of the sun, work is done on them and they acquire enhanced velocity. Thus on the whole the velocities of the solar molecules in consequence of the solar attraction tend to increase.

On the other hand there is a distinct loss of energy to which the molecules are exposed. As the velocities increase the encounters between the different molecules become more severe. After each such encounter the molecule vibrates with increased energy, obtained at the expense of the velocity of translation. The molecules are thus rendered more competent to impart energy to the surrounding ether, that is to say, they acquire an increased power of radiation. Thus we see that the fact of the sun's contraction translates itself with a tendency

to increased speed in the molecules. A portion of the energy thus arising is appropriated by the molecules and thus becomes fitted for the sustenance of the sun's heat.

So long as the sun shrinks in dimensions there will be an energy available towards the maintenance of its radiation. The termination of the supply will be reached when the sun has become so far solid that gravitation is no longer effective in reducing its volume at a sufficiently rapid rate.

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